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THE QUARTERLY REVIEW of BIOLOGY

TETRAPOLAR SEXUALITY

By JOHN R. RAPER

Department of Botany, The University of Chicago

INTRODUCTION

SEXUALITY and other phenomena associated with sexual processes in fungi have been intensively studied during a century of mycological research, and the elucidation of these phenomena has provided some of the most intriguing and significant contributions yet made by this branch of the biological sciences. The fungi exhibit a greater variety of sexual processes than any other group of organisms and encompass, often, to be sure, in simplified expression, the major types of basic sexual manifestations found in other plants and animals. Sexual processes in the fungi range from a simple apposition, in the more primitive groups, of male vs. female to a number of complex patterns, in the more advanced forms, in which sex per se appears to have been relegated to a secondary role by the superposition of additional determining factors (Raper, in press; Whitehouse, 1949a). The latter cases show unmistakable evidence of being highly evolved systems, but it is extremely difficult to rationalize the step-wise changes through which they could have evolved from simpler types.

Perhaps the most highly evolved of all sexual systems in fungi is that known as *tetrapolar sexuality* and found only among the Basidiomycetes; certainly it is the most intensively studied. During the period 1920–1940, numerous workers in Germany, Canada, France, and elsewhere minutely examined the developmental histories of scores of species belonging to the Hymenomycetes. Tetrapolar sexuality occurred in a majority of the species studied and only relatively minor deviations from the typical pattern were found throughout this group. A voluminous literature rapidly accumulated, and a number of excellent reviews summarized and integrated the vast amount of information dealing with the subject of tetrapolarity. Outstanding among the latter are the following: the section dealing with the Basidiomycetes in Kniep's *Die Sexualität der niederen Pflanzen* of 1928, various portions of Buller's *Researches on Fungi* (1924, 1931, 1933) and "The Diploid Cell and the Diploidisation Process in Plants and Animals, etc." of 1941, and Whitehouse's "Multiple-Allelomorph Heterothallism in the Fungi," of 1949. The available review literature, however, is wanting for a number of reasons. Kniep's review, although beautifully comprehensive at the time of its writing, appeared during the period of most intensive work and consequently lacks much later material which is pertinent to the basic problem of tetrapolarity. The reviews of Buller and Whitehouse are each primarily devoted to limited aspects of the problem and attempt no integrated treatment of all aspects of this unique pattern of sexuality. Finally, during the past decade, a few scattered observations have been reported which at first appeared as inexplicable in terms of the classic understanding of tetrapolarity

but which, upon confirmation and extension, now stand revealed as integral aspects of the tetrapolar pattern which surprisingly were overlooked in all of the earlier investigations. Continuing work along numerous lines indicated by these recently discovered aspects promises to provide significant information about a number of basic biological phenomena, in addition to a fuller understanding of the tetrapolar pattern per se.

Two considerations, therefore, prompt the present review. In the first place it will seek to present a comprehensive, integrated and up-to-date picture of the various ramifications of tetrapolar sexuality, and in the second place it will serve as the necessary basis for a projected series of experimental papers dealing with heterokaryosis in tetrapolar species and with certain consequences of these sterile nuclear associations. Numerous results of previously reported (Raper, 1951, 1952) but as yet unpublished work are included in anticipation of their detailed presentation in future publications. The pertinence of these recent findings to a full appreciation of the tetrapolar pattern warrants their inclusion in an otherwise general review.

I. LIFE CYCLE AND SEXUALITY IN THE HYMENOMYCETES

A single, relatively simple life cycle and developmental history is common to all species of the Hymenomycetes. The basidiospore, typically containing a single haploid nucleus or two haploid nuclei mitotically derived from a single meiotic product, germinates to produce a vegetative mycelium having only genetically identical nuclei. This haploid, vegetative mycelium is variously termed *primary mycelium*, *haplophase*, *monokaryotic mycelium*, *monokaryon*, or *homokaryon*.

Following an indefinite period of vegetative growth, the homokaryotic mycelium gives rise, through a sexual process termed *plasmogamy* and involving either an intramycelial (homothallic) or an intermycelial (heterothallic) association of compatible nuclei, to a new kind of mycelium characterized by paired nuclei, each pair known as a *dikaryon*. This type of mycelium, variously known as the *secondary mycelium*, the *dikaryophase*, the *dikaryotic mycelium*, and, for convenience though not strictly accurately, the *dikaryon*, is capable, like the homokaryon, of indefinite vegetative growth. In the growth of the dikaryotic mycelium, the nuclei of each pair undergo repeated, simultaneous, mitotic divisions, *conjugate divisions*, thereby providing one or more identical pairs of nuclei for each cell of the thallus. In many species, though by no means in all, clamp connections are regularly formed in the process of conjugate division.

Fruit bodies, *sporocarps*, are eventually produced as highly specialized outgrowths of the dikaryotic mycelium, each fruit body most probably originating through the extensive proliferation of a single mycelial cell (Brunswik, 1924; Oikawa, 1938). The dikaryotic mycelium, depending upon available substrate and environmental conditions, may remain alive and continue to grow for a period of years, furnishing material for perennial growth of a large fruiting body

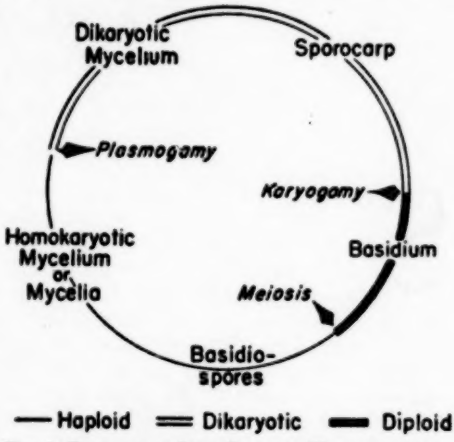

FIG. 1. DIAGRAM OF LIFE CYCLE OF HYMENOMYCETES

(woody bracket fungi) or producing a number of successive crops of fruiting bodies ("fairy rings"). Upon or within the sporocarp, a columnar layer of elongated, terminal, binucleate cells becomes differentiated from other components as the *hymenium*, the spore-bearing element. *Karyogamy* occurs within each initially binucleate cell, the *basidium*, with the two nuclei of the dikaryon fusing to form a diploid nucleus. Very shortly following its formation, the fusion nucleus, the only true diploid phase in the life cycle, undergoes two successive divisions, meiosis I and II, producing four haploid nuclei. Typically four spores are then formed exogenously upon the surface of the basidium, and a newly formed haploid nucleus migrates into each spore.

The life cycle is diagrammatically represented in Fig. 1. While a common, basic life cycle is

shared by all members of the Hymenomycetes, important variations are imposed at certain points in the cycle by different patterns of sexuality.

Three basic patterns of sexuality regularly occur in the Hymenomycetes: *homothallism;* and two types of heterothallism, *bipolarity* and *tetrapolarity*. According to a recent estimate (Whitehouse, 1949b), the three types of sexuality are distributed among the species of this group as follows: 10 per cent homothallic, 36 per cent bipolar, and 54 per cent tetrapolar. These three patterns of sexuality are randomly distributed throughout the many diverse forms comprising the Hymenomycetes. Tetrapolarity is known to occur in the related group, the Gasteromycetes.

In homothallic species, each basidiospore germinates to form a mycelium within which nuclei become associated in dikaryotic pairs. Either of two basically distinct modes of dikaryotization may operate here in specific cases. (1) Provided the mycelium is truly homokaryotic, i.e., contains only genetically identical nuclei, physiologically differentiated elements, perhaps adjacent cells of a single hypha or cells of different hyphae brought together by growth, may provide phenotypically compatible nuclei which establish the stable dikaryon. True homothallism of this sort has been established with certainty only in a few species of Hymenomycetes. Harder (1926), by means of the microsurgical isolation of uninucleate, penultimate cells of dikaryotic hyphae, has shown, for *Coprinus sterquilinus*, that dikaryotic associations were reestablished between the sister descendants of a single haploid nucleus. The occurrence of true homothallism here is not surprising, since it is the typical pattern of sexuality in a majority of all fungi and is known to occur in all other major groups. (2) In a number of known cases (Sass, 1929; Skolko, 1944), two nuclei which are genetically dissimilar are included in each basidiospore at the time of its formation. Each spore of this sort germinates to produce a mycelium which either is dikaryotic from the start or within which dikaryons are established shortly after germination. The term *secondary homothallism* has been applied to this condition, which involves the intrinsic association in the spore of genetically differentiated nuclei carrying compatible mating factors (Whitehouse, 1949a).

In bipolar and tetrapolar species, each basidiospore develops into a self-sterile mycelium which, grown alone, typically remains homokaryotic indefinitely.

In bioplar species, self-sterility and obligatory crossbreeding are imposed and controlled by incompatibility factors at a single genetic locus. Thus the diploid nuclei of a sporocarp of a bipolar species may be represented as A^1A^2, and at meiosis one-half of the haploid nuclei, hence subsequently the spores and the mycelial progeny, will contain incompatibility factor A^1, the other half, A^2. In contrasts between mycelial progeny, fertile matings leading to the establishment of a dikaryon will occur in those cases in which the two mates carry dissimilar factors. This pattern of factor segregation and cross-fertility obtains, with occasional deviations to be discussed below, within the limits of a single fruit body and its linear progeny. An extended allelomorphic series, however, occurs at the incompatibility locus in all bipolar species of the Hymenomycetes; and so far as is known, all alleles of the series are equivalent. Thus the haploid population of any given species comprises an extended series of distinct mating types, A^1, A^2, A^3, ... A^n, within which fertile matings will occur in all combinations of mycelia having different incompatibility factors. Multiple incompatibility factors also occur in all known tetrapolar species, and the details of the system as well as certain of its implications will be more fully discussed below in connection with tetrapolarity.

The most common and the most highly evolved pattern of sexuality among the Hymenomycetes is tetrapolarity, a pattern having no exact counterpart in any other group of organisms.

II. THE BASIC PATTERN OF TETRAPOLARITY

The discovery of heterothallism in the Basidiomycetes was made independently by Mlle. Marie Bensaude (1917, 1918), of France, in *Coprinus fimetarius*, and by Hans Kniep (1918), of Germany, in *Schizophullum commune*, during the latter part of World War I. Each, for the first time, recognized the significance of the presence or absence of clamp connections in basidiomycetous mycelia. Bensaude showed that monosporous mycelia, hence homokaryotic in the species with which she worked, remained devoid of clamp connections indefinitely and produced no fruiting bodies. Furthermore, in matings between certain mycelia, no clamps were formed, and such mates were interpreted as belonging to the same "sexual"

type. In other matings, however, typically clamped mycelia were produced, and these, under proper conditions, eventually fruited (Bensaude, 1917, 1918). These observations, including a detailed cytological study of the process of conjugate division, were almost exactly duplicated independently by Kniep (1918, 1920), who, however, carried the work far beyond a simple demonstration of heterothallism. It is to Kniep that we are indebted both for the first understanding of tetrapolarity and for the subsequent delineation of many of its major characteristics.

Basidiospores, and subsequently mycelia, of four distinct mating types are produced in approximately equal frequency by each tetrapolar fruiting body. Of the six possible contrasts between these four types, only two yield fertile matings, i.e., lead to the establishment of a dikaryotic mycelium and eventually to the formation of sporocarps; the other four combinations remain non-dikaryotic and sterile. This was interpreted by Kniep as resulting from the occurrence and independent assortment of allelic determining factors at two genetic loci:

$$AaBb \longrightarrow AB, Ab, aB, \text{ and } ab^{*}$$

Several different systems have been used by the many workers to designate and differentiate the incompatibility factors. The symbol, "*", will henceforth be used to indicate the use of the original nomenclature in citing the results of various workers where the system of designation differs from that adopted by the writer.

Matings will occur, in crosses between these four types, only in those combinations which will restore the double heterozygote, viz., $AB \times ab$ and $Ab \times aB$. Matings will not occur in any combination of mycelia having common factors at either *A* or *B* locus. At the time of the first description of this segregation and mating pattern, the designation of loci and alleles was necessarily arbitrary, e.g., the *A* factors could be distinguished, of course, from the *B*'s within a species, but not between species, and, similarly, if *AB* were arbitrarily assigned, *Ab* and *aB* could not be identified with certainty, since reactions between mycelia were commonly considered only in respect to the production of clamp connections.

During a period of a few years numerous workers in Europe and America confirmed the basic aspects of tetrapolarity as described by Kniep and extended them to many species of the Polyporales and Agaricales (Brunswik, 1924; Buller, 1924a, b; Hanna, 1925; Mounce, 1922, 1926; Vandendries 1922, 1923, 1925).

The regular production of four types of progeny led Kniep to inquire into the mechanics of the segregation of the incompatibility factors. This problem was approached by means of tetrad analysis in *Aleurodiscus polygonius*, a species of the Thelophoraceae in which the quartet of spores on each basidium is frequently discharged as a group, thereby facilitating the isolation of the individual spores in tetrad groups (Kniep, 1922). Two kinds of tetrads in respect to incompatibility factors were found to occur on the same fruiting body:

1) AB, AB, ab, ab^{*}
2) Ab, Ab, aB, aB^{*}.

To explain the occurrence of these two types of tetrads from identical diploids, Kniep postulated that the incompatibility loci *A* and *B* were carried on different pairs of homologous chromosomes, which were reduced and randomly distributed at the first meiotic division. In 1925, however, Funke and Hanna independently described a third type of tetrad which occurred in three other tetrapolar species, *Hyphaloma fasiculare*, *Collybia velutipes*, and *Coprinus lagopus:*

3) AB, Ab, aB, ab^{*}.

The following year Newton (1926) and Brunswik (1926) determined the relative frequencies of the three types of tetrads in *Coprinus lagopus* as (1) 9, (2) 8, (3) 25, and in *C. fimetarius* as (1) 29, (2) 27, (3) 37, respectively. Miss Newton, in seeking to rationalize the three types of tetrads, postulated a refinement of the earlier suggestion of Kniep, namely, that the homologous pair of chromosomes carrying one incompatibility locus was reduced at the first meiotic division, the pair carrying the other locus was reduced at the second division.

Comparable data for a number of species were reported during the following decade (Bohn, 1934; Kniep, 1928; Oort, 1930; Quintanilha, 1933, 1935). Two important facts were evident from these data: (1) the two kinds of two-type tetrads, one representing the parental combination, the other the non-parental combination, occurred in approximately equal frequency in each species; and (2) the frequency of four type tetrads varied widely in different species, ranging from 8.6 per cent in *Aleurodiscus polygonius* to 67 per cent in *Coprinus fimetarius*.

Dodge, in 1940, suggested that these results were explicable in terms of crossing-over and the subsequent delay of segregation of one or both pairs of factors until the second meiotic division. This explanation was suggested by the earlier demonstration in *Neurospora crassa*, by Lindegren (1933, 1936), that the order of characters transmitted in the linearly arranged ascospores directly revealed segregation at the first or second division and that the frequency of second-division segregation of any character was an accurate measure of the distance of the responsible locus from the centromere. Thus in a dihybrid cross, two- or four-type tetrads would be produced depending upon crossing-over. The meiotic products of the Eubasidiomycetes, unfortunately, are not neatly arranged in a linear order which provides a key to first-and second-division segregation, as are the ascospores of *Neurospora*. A comparable method of analysis for unordered tetrads, however, was independently derived in 1949 by Whitehouse (1949b), for the incompatibility factors of tetrapolar species, and by Perkins (1949), for non-linked allelic pairs in the smut fungi. The frequency of 4-type tetrads is given by the expression:

$$x + y - 3xy/2,$$

where x and y are the frequencies of second-division segregation of the two factors respectively. Thus if no crossing-over occurs between loci *A* and *B* and their respective centromeres, only 2-type tetrads are formed, with parental and non-parental types being of equal frequency; if a single cross-over occurs, either for *A* or *B*, a 4-type tetrad is produced; if single cross-overs occur between both loci and their centromeres, 2- and 4-type tetrads are produced in equal frequency; etc. More recently, Papazian (1951) has applied this method of analysis to *Schizophyllum commune* in mapping the incompatibility loci and a mutated locus linked to the *A* locus.

III. MULTIPLE ALLELOMORPHIC INCOMPATIBILITY FACTORS

Early in the work on bipolar and tetrapolar sexuality it was found that single fruiting bodies regularly produced progeny of only two or four mating types, respectively. Cross matings of progeny of different fruits of the same species, however, usually yielded far more fertile combinations than could be accounted for by the occurrence of the two and the four mating types characteristic of bioplar and tetrapolar species. The progeny of two sporocarps commonly were fertile in all interfruit contrasts, although the progeny of each sporocarp conformed exactly to the bipolar or tetrapolar pattern (Buller, 1930). Since the fruit bodies employed for these studies were collected directly from nature and frequently originated from widely separated localities, fruits of which the progeny were interfertile were described as belonging to different "geographical races" (Kniep, 1918, 1922; Brunswik, 1924). It soon became apparent, however, that remarkably little geography was requisite to the effect, as various authors have described complete interfertility between progeny of fruit bodies collected a short distance apart on the same log, on the same dung-ball, etc. (Brunswik, 1924; Hanna, 1925; Vandendries, 1923b).

It was thus established that an extended series of multiple allelomorphic incompatibility factors exists in nature and that intermycelial fertility is barred only in those cases in which the mates carry one or two factors in common.

In tetrapolar species of the Hymenomycetes, the number of physiologically distinct incompatibility factors demonstrable at both *A* and *B* loci appears to be a function primarily of the persistence and stamina of the investigator and only secondarily of any practical limitation of alternate states in nature. Two facts account for this seeming inversion of experimental limitations: (1) the multiple series at each locus is known to be extensive, hence, to distinguish how extensive, large samples would be required; and (2) the arithmetic of the effort is clearly against the investigator, for the contrasts required increase as the square of the number of individuals of the sample. Thus to determine the number of distinct *A* and *B* factors in a sample of 100 sporocarps, all four factors of each of which would be carried by two compatible progeny, would require 19,900 matings, a task which few would undertake lightly. A number of extensive studies along these lines have nonetheless been made.

Kniep (1928) reported complete interfertility between the homokaryotic strains of 15 different fruits of *Schizophyllum commune*, i.e., a total of 30 distinct factors at each of the *A* and *B* loci. Brunswik (1924) and Hanna (1925) obtained similar results from 13.5 and 7 fruits of *Coprinus fimetarius* and *C. lagopus*, respectively. Duplications of *A* or *B* factors, or both, however, have

commonly been reported in extensive studies of this sort. In 14 fruits of *Polyporus abietinus*, Fries and Jonasson (1941) found 23 *A* and 26 *B* factors of the 28 possible of each kind. All of these studies involved analyses of populations for which the samplings were largely made from widely separated points. Roshal (1950), in the examination of 12 fruits of *Schizophyllum commune*, all collected within a 70-acre tract of forest at Lake Geneva, Wisconsin, found 23 *A* and 21 *B* factors of the 24 possible of each. It is thus evident that the number of factors at each locus is large and that the dispersal of spores by wind allows very efficient randomization of these factors in the natural population.

Assuming completely random distribution of incompatibility factors and a low rate of mutation to new factors, Whitehouse (1949b) has estimated from the available data the minimal number of factors at each locus in natural populations as of the order of 100. Comparable values were arrived at for the single series in bipolar species. Recent work pertaining to the nature and frequency of "mutations" of incompatibility factors (cf. section IV below), however, may necessitate significant revision of the basic assumptions from which these estimates were derived. In any event the number is large, and the estimate of 100 may be used to illustrate certain implications of the tetrapolar, multiple incompatibility factor pattern. In a natural population having 100 distinct factors at each locus, 10,000 distinct homokaryotic mating types are possible; between the latter, 49,005,000 combinations, or slightly more than 98 per cent of all possible contrasts, would yield fertile matings. This pattern of obligatory cross-breeding allows almost unlimited opportunity for outbreeding while restricting inbreeding to 25 per cent, a device recognized by numerous authors (Mather, 1942; Papazian, 1951; Whitehouse, 1949b) as securing for a species the nearest approach to the maximal benefit to be derived from genetic recombination.

It is of interest here that in two gasteromycetous species, *Cyathus striatus* and *Crucibilum vulgare*, both members of the Nidulariaceae, a family characterized by a highly specialized method of spore dispersal but one which localizes dissemination markedly, only 9 of 36 possible factors were found in one species and only 14 of 60 possible in the other (Fries, 1940, 1943). These data, according to Whitehouse (1949b), indicate multiple allelic series of the order of 10 at each incompatibility locus in these forms.

The term "geographical race," as denoting a particular and unique combination of incompatibility factors, has been logically condemned by Vandendries (1929) and many later authors, since it has been repeatedly shown that the incompatibility factor constitution of two fruit bodies, except when originating from the same dikaryotic mycelium, is completely independent of the distances which separate them. Similarly, common incompatibility factors have frequently been demonstrated in fruits collected hundreds of miles apart. Either "incompatibility race" or "incompatibility stock" would be preferable to "geographical race," but these terms, also, suffer by their limited usefulness after they have been defined sufficiently precisely to be meaningful. Papazian (1950c) has designated a specific fruiting body and all of its linear progeny by the simple term "stock," a use which recognizes distinct genomes as well as probable, though not necessary, specific and unique combinations of incompatibility factors. The term "stock" will be used in this sense henceforth in the present account.

IV. "MUTATION" OF INCOMPATIBILITY FACTORS

Yet another complicating factor became evident early in the study of tetrapolar sexuality. Kniep (1923) first described, among the progeny of a single fruit body of *Schizophyllum commune*, monosporous mycelia which would mate with two of the four mating type classes instead of with only one expected class. This he interpreted as the result of mutation at one or the other of the incompatibility loci, with the altered *A* or *B* factor allowing for fertility in otherwise unexpected combinations. The same phenomenon was subsequently shown to occur in other bipolar and tetrapolar species, by Zattler (1924) in *Collybia velutipes*, by Vandendries (1924) in *Coprinus radians*, by Newton (1926) and Hanna (1925) in *Coprinus lagopus*, by Brunswik (1924) and Quintanilha (1935) in *Coprinus fimetarius*, etc. Furthermore, in many species, occasional mycelia which at first were apparently homokaryotic and displayed no anomalies of mating behavior, would become spontaneously dikaryotized upon aging. This phenomenon has likewise most frequently been interpreted as the result of spontaneous mutations of incompatibility factors.

A number of causes, as pointed out by White-

house (1949b), might well lead to results which are difficult or impossible to distinguish from those caused by true mutations. Chief among these extraneous causes are various means of contamination by spores, oidia, or hyphal fragments from other cultures or fruit bodies in the laboratory. Particularly difficult to rationalize otherwise are the relatively numerous reports of spontaneous dikaryotization, which, in tetrapolar species, would require mutations of both loci, an occurrence of exceedingly infrequent expectation. Whitehouse has taken the rather severe attitude that those reports are not trustworthy in which such possible contamination has not been rigorously ruled out and the "new" factors shown to differ from those carried by nearby cultures. The similarity of results in "trustworthy" and "non-trustworthy" studies, however, tend to argue against contamination as a major cause of "mutation" in the latter category.

Perhaps the most extensive analysis of mutations of incompatibility factors was provided in 1928 by Kniep. During a period of several years, numerous mutations of *Schizophyllum commune* had been observed, and in most cases the actual incompatibility factor constitution had been determined in respect both to the "normal" factors carried in cultures in his laboratory and to the "new" factors of the mutants themselves. From these extended observations he was able to draw certain significant conclusions. (1) Mutated incompatibility factors were most commonly apparent in newly isolated monosporous cultures. Fully established homokaryotic mycelia appeared to be quite stable, with the exception of very rare cases of spontaneous dikaryotization. (2) Mutations appearing among the progeny of any particular dikaryon, having of course its own characteristic four factors, tended to belong to a few, favored "new" factors. (3) Further mutations of the "new" factors occurred with approximately the same frequency as the originals and among these "new-new" factors, the original, or "wild" factors were common. (4) Mutations occurred much more frequently at one locus than at the other, the incidence at the A^* locus being about 2 per cent, at the B^* locus, about 0.5 per cent.

These conclusions of Kniep suggested to Papazian (1951) the possibility that changes of incompatibility factors resulted from crossing-over between two or more pairs of genes, closely linked and determining a single physiological character, rather than from true genic mutations. After developing a technique for the isolation of tetrads of *Schizophyllum commune* (Papazian, 1950a), a technique which Kniep considered impractical because of the small size of the spores, he subjected the hypothesis of crossing-over to tetrad analysis and to an analysis of random samples of spores (Papazian, 1951). The results of both kinds of analysis are of significance.

A total of 300 monosporous mycelia were tested for mating type, employing testing procedures which revealed changes of incompatibility factors at both loci. In the total sample, nine mycelia having "new" *A* factors and one carrying a "new" *B* factor were found. These represent mutation frequencies of 3 per cent for the *A* factor and 0.33 per cent for the *B* factor, results which were in fair agreement with those of Kniep. Of the nine mutated *A* factors, there were two pairs which were identical. Thus the "new" kinds of incompatibility factors which were recovered totalled seven *A* and one *B*. Of these eight "mutants," six, all showing change of the *A* factor, occurred in the progeny of matings of a single stock, namely, between mycelia carrying factors A^1 and A^3; the remaining two, one *A* and one *B*, were derived from two additional stocks. These data, concerning the stock ancestors of the various "mutants," were omitted from the published account of this work but are tabulated in detail in the unpublished dissertation (Papazian, 1950b).

The mating types of individual members of about 25 tetrads were determined. Of these, three tetrads are of outstanding significance. The first was an incomplete tetrad taken from the mating $A^1B^2 \times A^2B^1$ and comprised spores, and subsequently mycelia, A^2B^2, a^8B^1, and a^7B^2 (the lower case letters represent mutated factors; a^7 and a^8 are identical to two previously isolated mutants, a^1 and a^2, respectively). The fourth spore was either not produced or was lost in the procedure of isolation and presumably was of type A^1B^1 or A^2B^1. Various mycelia carrying "mutants" of the *A* factor were cross-mated, and two additional tetrads of interest were recovered from these crosses:

$a^8B^2 \times a^7B^1 \rightarrow A^1B^2, A^2B^2, a^8B^1, a^7B^2$

$a^8B^2 \times a^7B^1 \rightarrow a^7B^2, a^8B^1, A^1B^1, (A^2B^2$?, lost).

In both of these tetrads, from crosses between "mutant" types, the "new" factors are identical to the wild *A* factors from which "mutants" a^7 and a^8 were originally derived.

From these limited data on tetrads it appears to be highly probable that "mutants" always originate in pairs. The further fact that in each of the two cases in which both were recovered they were different *and* interfertile constitutes eloquent evidence that the "mutations" involved exchanges of subunits, i.e., crossovers, rather than independent genic mutations. Thus there is little doubt that the incompatibility factor at the *A* locus in *Schizophyllum* is a group of two or more closely linked genes which together determine a single physiological character, the mating type, and between which crossing-over occurs with a frequency of 2–3 per cent to yield "new" factors. Papazian, in discussing the results outlined above, suggested different possible relationships between the genes comprising the *A* factor to account for the large number of alternate factors occurring in the natural population. A multiple series of 100 distinct incompatibility factors could be rationalized as comprising 10 alleles at each of two loci, five alleles at each of three loci, or by other combinations. Perhaps the weight of long-held acceptance of multiple allelic series in connection with incompatibility factors in the Hymenomycetes discouraged serious consideration of the alternate explanation of a number of very closely linked loci each with only two alleles.

In actuality, Papazian's data, including those to be found in his dissertation but not included in his published papers, do indicate something of the minimal genetic structure of the complex designated the *A* incompatibility factor. From the summed results of random spore and tetrad analyses, it will be recalled, six "new" factors were isolated and identified among the progeny of intrastock crosses, $A^{1-} \times A^{2-}$. These six "new" combinations plus the two originals, A^1 and A^2, total eight different combinations known to occur within a single stock. Since only two alleles at any locus can possibly be involved within a single stock, the minimal number of distinct loci which could possibly give eight different combinations would be three. The possible number of combinations between two alleles at each of several loci would be represented by the function 2^n, where n is the number of loci involved: (1 locus) 2 factors, (2) 4, (3) 8, (4) 16, etc. Further consideration of his data, however, reveals the extreme improbability of only three loci being adequate to explain his findings. Whatever the number of loci involved, the total linkage distance for the complex can only be approximately 1.5 crossover units, i.e., one-half the recombination frequency. Assuming three loci with equal distances between adjacent pairs, the expected frequency of crossovers occuring in both segments would be about 5×10^{-4}, and double crossovers would be required for two classes of the eight possible combinations. Thus the probability of finding all eight types, including both double crossover types, in a total sample of less than 300 (the data do not indicate the number of mycelia of other stocks tested) would be vanishingly small (10^{-4}), and it can only be concluded that a larger number of loci are involved. For four loci, the two originals, six single crossover, six double crossover, and two triple crossover combinations make a total of 16 possible incompatibility factors. It can therefore be concluded that four loci comprising incompatibility factor *A* is the minimal number which can reasonably account for the six classes of observed recombinations.

An estimation of the maximal number of loci hinges, of course, upon the probability of the recovery from a sample of 300 of all possible types of single crossover combinations. The distribution of original and recombined factors as reported by Papazian was as follows:

Factor	A^1	A^2	$a^1(a^7)$	$a^2(a^8)$	a^3	a^4	a^5	a^6
Original	1	1	—	—	—	—	—	—
Recomb.	2	2	2	2	1	1	1	1

For exploratory purposes, two simplifying assumptions may be made: (a) that all interloci linkage distances are approximately equal; and (b) that the recombination classes represent the paired products of genetic interchange, a relationship known to exist between a^7 and a^8 and, in recombination, between A^1 and A^2. The findings tabulated above would then fit 4 to 10 loci within limits of 5 per cent probability. If, however, one or both of these assumptions be inadmissible, the probable number of loci would be increased. (This evaluation is based upon calculations of probability and "fit" by Dr. Herbert D. Landahl of the Dept. of Mathematical Biology, University of Chicago.) The first assumption is admittedly suspect from the start, for more than half of the recombinants considered here, A^1, A^2, $a^1(a^7)$, and $a^2(a^8)$, represent crossovers in the same intergene segment, and the original work of Kniep indicated that certain "mutants" occurred more frequently than others. The sample studied by Papazian was too small to resolve these uncertainties, and the possibility of

multiple allelic series at one or more of the involved loci could not in any event be excluded by investigation of a single stock. His findings, however, do indicate a sufficiently extended series of distinct loci to predict the total number of *A* factors in the natural population without necessary recourse to multiple allelic series. The requirement of about 100 *A* factors in natural populations, as estimated by Whitehouse, would be satisfied by single pairs of alleles at each of seven loci, which would make possible 128 different combinations.

The considerations above apply directly only to the incompatibility factors at the *A* locus, or more properly the *A*-incompatibility chromosomal segment, of a single species, *Schizophyllum commune*. That the multiple factors at the *B* locus in this particular species, and at both the *A* and *B* loci in other species, are similarly constituted is strongly indicated by the remarkable uniformity of multiple factor series throughout the Hymenomycetes, both in respect to indicated extent and to intraseries, physiological behavior. Investigated species of Gasteromycetes would appear to possess less extended series of factors, which, within the framework of the interpretation outlined above, would require no more than four loci for each factor.

Spontaneous dikaryotization, the second phenomenon frequently interpreted as the result of mutation of incompatibility factors, can be clearly distinguished from the changes discussed above. The conversion of a well-established monosporous mycelium, initially displaying the normal mating behavior of a homokaryon, to a typical dikaryotic mycelium cannot result solely from crossing-over at meiosis. This anomaly will be discussed more fully in connection with other irregularities of the process of dikaryotization.

V. INTERTYPE MATING PATTERN

The great preponderance of the work on tetrapolar sexuality until quite recently was concerned almost exclusively with intermycelial fertility, and primary attention was consequently directed to the occurrence or failure of dikaryotization in countless matings between homokaryotic strains. Thus all of the early workers, in numerous tables of crosses, scored each particular contrast as (+) or (−). If any other reactions were observed, they must have been considered as of little interest or significance.

In 1933, Vandendries and Brodie (Brodie, 1936a; Vandendries and Brodie, 1933) gave the first detailed description of a reaction other than dikaryotization between homokaryotic mycelia. They found that in matings of homokaryotic mycelia of *Lenzites betulina* having one incompatibility factor in common, i.e., $ab^1 \times a^1b^{1\,8}$, etc., an area containing only a few hyphae was evident along the line of contact between the two mycelia. Into this area, they reported, few hyphae grew from either of the contrasted mycelia, and certain of these hyphae, particularly aerial filaments, were bent back upon themselves as though an actual repulsion had occurred. They termed this mutual antipathy of the two mates "barrage sexuel" and presented evidence which indicated that the effective agent of inhibition of growth and hyphal repulsion was mitogenetic, or Gurwitsch, rays (electromagnetic radiations in the ultraviolet range). This reaction, previously noted by Brunswik (1924) and Oort (1930), has since become recognized as a typical response in certain types of incompatible matings in many other tetrapolar species, and it is only recently that any explanation other than inhibition by mycelial emanations has been advanced.

A second type of monosporous mycelial interaction other than dikaryotization and quite distinct from "barrage" was briefly described by Kaufert (1935) for *Pleurotus corticatus*. In matings between certain incompatible strains both of the interacting mycelia henceforth showed retarded and depressed growth, and the individual hyphae of such mycelia were described as abnormal. The incompatibility factor composition of the mycelia giving this reaction was not designated.

In 1950, these reactions were shown to be an integral part of the pattern of tetrapolar sexuality by Papazian (1950c). In an extended investigation of *Schizophyllum commune*, it was found in matings between mycelia of each mating type with those of other mating types that predictable reactions occurred in each kind of contrast and that these reactions exactly reflected specific intermycelial incompatibility factor combinations. Thus when matings were made in all possible combinations between mycelia of all four mating types of a single stock, e.g., $A^1A^2B^1B^2$, the following reactions regularly occurred:

(1) Unlike factors at *A* and *B* loci, $A^1B^1 \times A^2B^2$ and $A^1B^2 \times A^2B^1$, permit reciprocal dikaryotization of both participating mycelia.

(2) Unlike factors at the *A* locus and like factors at the *B* locus, $A^1B^1 \times A^2B^1$ and $A^1B^2 \times A^2B^2$, yield

a narrowly limited heterokaryon along the line of contact between the two mated mycelia. This region of heterokaryotic mycelium, in all probability, is the "barrage" described by Vandendries and Brodie (1933). This heterokaryon is relatively unstable and rapidly sectors into the component homokaryons upon subculturing.

(3) Like factors at the *A* locus and unlike factors at the *B* locus, $A^1B^1 \times A^1B^2$ and $A^2B^1 \times A^2B^2$, yield a reciprocally constituted heterokaryon throughout both participating mycelia. This heterokaryon, macroscopically characterized by a depressed habit and little aerial growth, was named "flat" by Papazian. The heterokaryon can be subcultured from any point on either of the two mycelia after their conversion, it is usually completely stable, and it may be repeatedly subcultured without apparent change.

(4) Like factors at *A* and *B* loci, $A^1B^1 \times A^1B^1$, etc., usually shows no reaction, or at most a very limited mutual overgrowth of the two mycelia. Using several mutant characters as genetic markers, it has been conclusively shown that there is at least no extensive interchange of nuclei between mates of the same mating type (Papazian, 1950c; Raper and San Antonio, unpub.).

Interactions of types (2) and (3) above will be more fully characterized and certain consequences of their physiological behavior will be set forth in a later section.

The pattern of intertype reactions listed above is now known not to be a specific and unique characteristic of *Schizophyllum*. Preliminary results of a survey being conducted in this laboratory have shown an exactly comparable pattern in *Lenzites betulina*, *Collybia velutipes*, and in each of several as yet unidentified tetrapolar members of the Thelephoraceae and Polyporaceae. The exact pattern varies appreciably in different species, as regards the relative size of the "barrage" region and the habit of "flat" mycelia as compared with normal homokaryotic and dikaryotic mycelia. The extremes of variation are shown by *Lenzites betulina*, in which the common-*A* heterokaryon can be distinguished from homokaryotic mycelia only with difficulty, and by two members of the Thelephoraceae, in which the mycelia of common-*A* matings remain very small and aberrant. Only a single tetrapolar species so far examined, *Pleurotus ostreatus*, failed to produce the common-*A* heterokaryon. In this species, each of the two mycelia in common-*A* mating retained its homokaryotic integrity as revealed by mating tests (Raper and San Antonio, unpub.).

In view of the tremendous amount of work which has been done on tetrapolar sexuality by extremely careful and conscientious people, it is very surprising that the clear-cut intertype mating pattern should not have been recognized for three decades. Only two possible reasons for the oversight can be advanced and it must be admitted that singly or together, they, hardly appear to constitute an adequate explanation. (1) Procedures reported in most cases employed culture tubes for mating, small masses of mycelium being removed for examination for clamps, etc. Under these conditions the two homokaryotic mycelia would largely cover the exposed surface prior to their complete heterokaryotization and the establishment of a peripheral fringe of pure heterokaryon. (2) In most of the investigations for which sufficiently detailed procedures are given to permit comparison, much richer media were employed than that used by Papazian, who adopted, with only slight modification, a basic medium which had been developed over a period of time by the author as one supporting reasonable growth of practically anything from aquatic Phycomycetes to wood-rotting Basidiomycetes. It has since been repeatedly observed (Raper and San Antonio, unpub.), in comparing certain metabolic aspects of heterokaryons, homokaryons, and dikaryons, that the characteristic cultural differences tend to disappear in rich media, particularly in those containing large amounts of complex natural products such as peptone, yeast extract, and malt extract. The use of rich media in culture tubes for matings, in the absence of reasons a priori to look for reactions other than dikaryotization, might account for the fact that these very significant and useful responses were so long overlooked.

VI. DIKARYOTIZATION

The absence of sexual organs on the mycelia of most of the Basidiomycetes was well established long before the discovery of the sexual processes in this group. The work of Kniep and Bensaude, as well as that of others in the first years following the discovery of heterothallism, indicated a somatic sexual process in which reciprocal fertilization somehow initiated the dikaryotic phase. Numerous authors have subsequently contributed to the present understanding of the process of dikaryotization. Preëminent among these was the late

Prof. A. H. R. Buller, who spent a number of years investigating various aspects of this problem (Buller, 1924b, 1931, 1933, 1941). The basic material for the following account is largely drawn from his work.

A number of different means are known to bring compatible nuclei into juxtaposition, such as fusion between germ tubes of germinating spores, conjugation of spores, "spermatization" of homokaryotic mycelia by uninucleate oidia, or fusion between two fully established homokaryotic mycelia. The processes of cellular union and nuclear behavior differ only in detail in the three cases, and the last, reaction between two established mycelia, may be used to illustrate the basic interactions.

When two compatible mycelia grow together, varying numbers of hyphal fusions are formed between individual vegetative cells of the two plants. Within the binucleate cells thus formed either of two patterns of nuclear behavior may ensue. (1) One or both of the two compatible nuclei may migrate so that they approach each other, become permanently associated, and together migrate into a hyphal outgrowth of the fusion cell, thus initiating a mycelium which is dikaryotic from the start. (2) Each of the two nuclei in the fusion cell may divide, a daughter nucleus of each migrating across the cytoplasmic bridge to become permanently associated with the non-migratory daughter of the other. The "invading" nucleus is stimulated to rapid division, for once within the compatible mycelium, daughter nuclei swiftly migrate throughout the preexisting mycelium, dividing in passing in many individual cells along the way and leaving daughter nuclei behind in association with the original inhabitants. The two mated, compatible homokaryotic mycelia are thus converted into a single dikaryotic mycelium. As daughters of the original invading nucleus reach the terminal cells of the preexisting mycelium, dikaryons are formed which henceforth divide conjugately as the hyphae continue to grow. In a majority of cases, clamp connections are formed in association with conjugate division.

Clamp connections on the hyphae of basidiomycetous fungi were first observed about a century ago (Hoffmann, 1856) and soon came to be considered a mycelial characteristic of members of this group. In 1915–1918, Kniep (1915) and Bensaude (1918) worked out the mode of formation of clamp connections cytologically and revealed the relationship between this process and that of conjugate division. The true significance of the dikaryon and the associated clamp connections were fully appreciated shortly thereafter, when Kniep and Bensaude established the fact of heterothallism in the Basidiomycetes. The typical relationship of clamp connections to the developmental cycle was rapidly established: in most species clamp connections are always found to be associated with conjugate division in dikaryotic mycelia and are lacking in homokaryons. Numerous exceptions to this general rule, however, are known. The homokaryotic mycelia of *Stereum hirsutum* (Kniep, 1918) and *Coprinus narcoticus* (Brunswik, 1924) may possess clamp connections, whereas the dikaryotic mycelia of *Corticium bombycinum*, *Armillaria mellea*, and *Calocera viscosa* do not (Kniep, 1913, 1918).

Dikaryotization, termed diploidization by Buller and many other authors, most typically occurs between two compatible homokaryotic mycelia, but there are a number of other circumstances of considerable significance in which dikaryotization may occur regularly or sporadically. The different modes of dikaryotization and the phenomena associated with them reveal a wide range of variation of this basic process in tetrapolar species.

1. *Homokaryon* × *homokaryon*

(a) *Legitimate*, e.g., $A^1B^1 \times A^2B^2$. Matings between two homokaryotic mycelia having unlike incompatibility factors at both loci are termed "legitimate," and from such matings much of the basic information on dikaryotization has been derived. A brief consideration of experimental findings relating to a few aspects of dikaryotization will serve both to clarify the process itself and to illustrate the characteristics of the dikaryon and the dikaryotic mycelium.

The rate of nuclear migration within a compatible mycelium, following the entry through hyphal anastomoses, was studied in detail by Buller and certain of his students in *Coprinus lagopus* (Buller, 1931). The basic experimental procedure employed was to introduce a small inoculum of a homokaryon (or dikaryon, see below) at the periphery of a compatible homokaryotic mycelium which had been growing for some days previously and then to determine the interval which elapsed prior to the appearance of clamp connections, a reliable criterion of dikaryotization in this species, at several points around the

periphery of the original homokaryon. The relationship of the time of appearance of clamp connections vs. distance from inoculum was determined in a number of different matings. The rate of migration of dikaryotizing nuclei was found to vary in different experiments from 0.79 to 1.45 mm./hr. and averaged approximately 1.0 mm./hr. Further experiments, in which central or peripheral portions of the preexisting mycelium were cut away prior to inoculation of the compatible strain, showed that migration could occur either through the older, central portion or through the peripheral fringe of the mycelium via hyphal fusions. It was also shown that relatively few invader-daughter nuclei were left behind in the older portions of the mycelium when this route of migration was taken.

The maximum rate of nuclear migration observed, 1.45 mm./hr., must be considerably lower than the actual rate for the following reasons: (1) no straight path exists, but migration must proceed through a three-dimensional network which could easily double the distance travelled, and (2) establishment of initial intermycelial hyphal connections must require at least a few hours, an appreciable factor in 30–60 hour experiments. The actual rate of migration could therefore be two or three times as great as the measurements would indicate. Such a rate of migration is truly remarkable in view of the fact that a large number of nuclear divisions, of the order of 100 per cm., must also be accomplished in transit, each in a matter of 5 to 10 minutes including migration to the next division site. There is also the further matter of approximately 150 septa per cm. to negotiate. Wahrlich, in 1893, reported that small, centrally located pores in the cross walls of septate hyphae allowed cytoplasmic continuity between contiguous cells. Lehfeldt (1923), in studying the specific problem of the migration of dikaryotizing nuclei, however, came to the conclusion that septal pores were either absent or of inadequate size to permit nuclear passage and that dissolution of a portion of the septum was therefore essential. Buller (1941) considered septal dissolution too time-consuming and felt that nuclei must be able to pass freely from cell to cell, a conclusion now readily confirmed by phase microscopy—the nucleus approaches a septum and squeezes through the pore, a process which requires a minute or so.

The dikaryotic cell is functionally a diploid cell, reflecting in its morphology and physiology the interactions of its two component nuclei as though they constituted a single diploid unit. Thus the dikaryotic mycelia resulting from crosses between variously differentiated homokaryons may show complete or incomplete dominance of the distinguishing characteristics. In *Collybia velutipes*, Zattler (1924) found completely colored dikaryons to result from the mating of colored and nonpigmented homokaryons. In *Peniophora allescherii*, dikaryons resulting from matings of normal and mutant mycelia displayed the growth characteristics of the mutant parent and the conidial habit of the normal parent (Nobles, 1935). The fruiting bodies and mycelia of *Panus stypicus* of North America are luminescent, whereas the same species in Europe lacks this character; Macrae (1942) has shown this difference to be due to a pair of alleles at a single locus, and in the heterozygous dikaryon the gene for luminescence is completely dominant. In this laboratory (Papazian, 1951; Raper and San Antonio, unpub.), numerous morphological and biochemical mutants of *Schizophyllum commune* have been shown to be completely recessive in dikaryotic combination with wild type. Exceptions have been observed with other morphological mutants; dikaryons may exhibit the mutant characters, but only in highly diluted expression.

In most cases, in matings of homokaryons directly derived from fruit bodies collected from nature, dikaryotization is reciprocal, with both of the two participating mycelia assuming the same morphological characteristics subsequent to the establishment of the dikaryon. This follows from the fact that the nuclear components of all portions of the same dikaryon are identical.

Dikaryotization, however, is not necessarily always reciprocal among the progeny of wild fruits. Greis (1942) found such an anomalous situation to obtain in *Solenia anomala*, the progeny of which consisted equally of small and large mycelia, with dikaryotization occurring only in large mycelia when mated with small, i.e., small dikaryotized large, providing dikaryotizing nuclei but accepting none from the large mate. [The elaborate interpretation developed by Greis (1942) and Hartmann (1943), sexual differences between *A* (large, ♀) and *a* (small, ♂) and sterility factors at a second locus, *B*, would appear to be unnecessary to explain the deviations here from the typical tetrapolar pattern.] Numerous other cases of non-reciprocal dikaryotization have been reported: in *Coprinus sphaerosporus*, by Dickson

(1935); in *Coprinus fimetarius*, by Quintanilha and Balle (1938, 1940); in *Schizophyllum commune*, by Roshal (1950), who found the anomaly in one-half the progeny of one wild fruit; in *Collybia velutipes* and in several unidentified species of the Thelephoraceae, by Raper and San Antonio (unpub.); and in the Gasteromycete, *Cyathus stercoreus*, by Brodie (1948), who coined the term "unilateral diploidization."

Unilateral dikaryotization is emerging in current studies (Papazian, 1950c, 1951; Raper and San Antonio, unpub.) as of very common occurrence in matings involving induced mutants. The inability to accept nuclei in dikaryotization is now known, from studies with a wide range of induced morphological mutants, to be a corrollary effect of the mutation of any one of a number of genes, and the inhibition in certain cases is not absolute. Since many of these mutants originate under conditions which may occur with reasonable frequency in nature (cf. section VIII), it is possible that these apparent anomalies may, after all, be resolved as an expected part of the tetrapolar pattern.

(b) *Illegitimate*, e.g., $A^1B^1 \times A^2B^1$. Any combination of homokaryons having a common incompatibility factor which yields a dikaryotic mycelium is termed "illegitimate." The validity and prevalence of matings of this sort, which have been reported in a number of species, is extremely difficult to assess properly. The most important confusing factors here are the regular occurrence of a low percentage of "mutant" incompatibility factors, and the ease with which these can be confused with illegitimate mating behavior in studies in which both types of deviation might be considered as incidental.

Illegitimate matings between homokaryons were studied in some detail in various species of *Coprinus* by Vandendries (1923a, b), by Brunswik (1924), who termed such matings "Durchbrechungskopulations," and by Quintanilha (1935). Brunswik found that illegitimate matings occurred only in contrasts having common *B* factors, i.e., $AB \times aB$ and $Ab \times ab^{6}$, never between mates having common *A*'s, i.e., $AB \times Ab$ and $aB \times ab^{6}$. This pattern was confirmed by Quintanilha, who extended his observations to a cytological and genetic study of illegitimate matings. He was able to obtain fruits of the mating $AB \times aB^{6}$ and showed that only spores of the two parental types were produced, either in mixed tetrads, 2 *AB* and 2 *aB*, or in tetrads having only a single type, 4 *AB* or *aB*. It would thus appear, in this case at least, that nuclear fusion and reduction did not regularly precede the formation of basidiospores. Too few cases which were actually known to involve illegitimate matings have been adequately studied to permit an accurate evaluation of the role of the phenomenon in tetrapolar sexuality.

2. *Spontaneous Dikaryotization*

E.g., $A^1B^1 \rightarrow (A^1B^1 + A^xB^x)$. The spontaneous conversion of homokaryons, which initially displayed only normal mating behavior, to dikaryotic mycelia in continued culture has been reported as occurring in low frequency in many species. Here again, as with the illegitimate matings above, it is difficult to assess properly the role of this anomaly, since to its undoubted occurrence must be added those cases in which chance contamination of established mycelia by spores, oidia, etc., have been incorrectly interpreted. Accounts of spontaneous dikaryotization have been given by Kniep (1920) and Papazian (1951) for *Schizophyllum commune*, by Vandendries (1925a, b, 1927) for *Coprinus radians* and *C. micaceus*, and by Hanna for *Coprinus lagopus* (1925, 1928).

In only two cases have incompatibility factor analyses of the newly formed dikaryon been made, and *Schizophyllum commune* was used in both of these analyses, by Kniep (1929) and Papazian (1951). In the latter case, clamp connections appeared spontaneously upon each of the four isolated mating types of a single stock, $A^3A^4B^3B^{4\,6}$, which had been kept at room temperature, whereas the corresponding subcultures kept at 3-5°C. remained homokaryotic. The mating type of each of the originals had been determined and monosporous progeny of one of these, A^3B^3, following its dikaryotization, were shown by matings with known test mycelia to be: A^3B^3, A^4B^3, A^3b^3, $A^4b^{3\,6}$. The "new" nucleus must therefore have been A^4b^3. Although A^4 was the alternate factor of the parental stock and b^3 was shown to be identical with B^1 of a different stock in abundant culture in the laboratory at the time, contamination appears to be very unlikely, inasmuch as the particular strain A^4B^1 had not been previously constituted by interstock crosses. An additional monosporous mycelium, dikaryotized before initial typing, gave essentially the same pattern: A^3B^4, A^3b^3, A^4b^3, $A^4B^{4\,6}$. Each of these cases involved the alternate *A*-factor of the parent stock

and a *B* factor of unknown origin. It is interesting that Kniep had found exactly the same situation in an earlier case of spontaneous dikaryotization in the same species.

Spontaneous dikaryotization, however, is of relatively rare occurrence, and it would appear that certain stocks have a predilection for the abnormality. Instances in which it has been sought among normal strains argue aganist all homokaryons being equally disposed. Thus homokaryotic mycelia have been reported to retain their integrity for considerable periods: for three years in *Coprinus lagopus*, by Hanna (1928); for five years in *Fones pinicola*, by Mounce (1929); and for five years in *Schizophyllum commune*, for Papazian's original types of stock $A^1A^2B^1B^2$ (Raper and San Antonio, unpub.). Nothing definitive is known of the underlying mechanism of spontaneous dikaryotization, but the very limited information available might indicate some sort of delayed expression of irregularities in the meiotic process. More intensive investigation of stocks showing the anomaly is clearly needed.

3. *Haploidy*

The production by monosporous mycelia, which by all known criteria remain indefinitely homokaryotic, of fruit bodies having progeny of only the same mating type as the parent has been described in a large number of tetrapolar species. These include *Armillaria mucida* and *Collybia velutipes* (Buller, 1924b; Zattler, 1924), *C. tuberosa* and *Typhula erythropus* (Kniep, 1922), *Panaeolus campanulatus* (Vandendries, 1923a), *Coprinus ephemerus* (Brunswik, 1924), *Fomes pinicola* (Mounce, 1929), *Coprinus lagopus* (Hanna, 1928), *Peniophora ludoviciana* (Biggs, 1938), etc. In most species the haploid fruits are smaller and show various abnormalities as compared to normal fruits, though in a few cases they may be in distinguishable.

Although the end result is the same in either event, two different patterns of nuclear behavior have been established in fruiting haploids. In *Schizophyllum commune* (Kniep, 1928) and various species of *Mycenia* (Kühner, 1926; Smith, 1934), a single haploid nucleus enters the young basidium, divides twice, and one of the daughters eventually enters each of the four basidiospores. In other haploid forms, including *Coprinus lagopus* (Chow, 1934), certain other species of *Mycenia* (Smith, 1934), and *Peniophora ludovinciana* (Biggs, 1938), two haploid nuclei enter the developing basidium and fuse. Following two successive divisions of the fusion nucleus, the four daughter nuclei are distributed singly among the four basidiospores, as in the preceding case. In some species showing both patterns of basidial development, only two, rather than the normal four, basidospores are produced in the haploid forms (Smith, 1934).

A number of haploid strains have been shown to be quite stable in culture; propagation by basidiospores for five and ten generations, showing no alteration of original characteristics, has been described by Zattler (1924) and Hanna (1928) in *Collybia velutipes* and *Coprinus lagopus* respectively. Furthermore, haploidy has been indicated in a few cases in nature, e.g., Bauch (1926) reported 2- and 4-spored varieties of *Hygrophorus virgineus*—haploid and dikaryotic forms, respectively, as judged by initial nuclear content of basidia—growing in distinct fairy rings in the same vicinity.

Haploidy, like the other anomalies described above, is too imperfectly known as yet to allow its integration into the total pattern of tetrapolarity. Whatever the basic mechanism and function, however, it does serve the essentially asexual function of the continued propagation of occasional strains.

4. *Homokaryon × dikaryon (The Buller Phenomenon)*

E.g., $(A^1B^1 + A^2B^2) \times A^1B^1$.

The dikaryotization of homokaryotic by dikaryotic mycelia was first described by Buller (1930, 1931) in *Coprinus lagopus*, has since been reported in a number of species, and is now established as a regular mode of dikaryotization in most bipolar and tetrapolar species. In 1937 it was named the "Buller Phenomenon" in honor of its discoverer by Quintanilha. More recently, Papazian (1950c) suggested the somewhat more convenient term, "di-mon matings," an abbreviation of "dikaryon × monokaryon matings." The basis for the Buller Phenomenon is the strong tendency for mycelia of all kinds of a given species to form hyphal fusions whenever they come into contact with each other. Once hyphal bridges are formed, nuclear migration may occur as in matings between homokaryons.

There are three types of di-mon matings in respect to the relationship of the incompatibility factors in the two mycelia. These were termed

"legitimate" and "illegitimate" by Buller, depending upon the presence or absence in the dikaryon of a nuclear type with no incompatibility factors in common with that of the homokaryon. The "legitimate" class of Buller was subdivided by Papazian (1950c) into (a) "compatible" and (b) "hemicompatible" for cases in which both or one nuclear type in the dikaryon are compatible with that of the homokaryon; for the "illegitimate" class the term "non-compatible" was proposed. A partial synthesis of the two terminologies will be adopted here as best serving the dictates of clarity. Thus the modes of dikaryotization of homokaryons by dikaryons may be defined and illustrated as follows:

Legitimate di-mon matings

(a) Compatible, e.g., $(A^1B^1 + A^2B^2) \times A^3B^3$, in which both nuclear types of the dikaryon are compatible with that of the homokaryon.

(b) Hemi-compatible, e.g., $(A^1B^1 + A^2B^2) \times A^1B^1$, in which one nuclear type of the dikaryon is compatible with that of the homokaryon.

Illegitimate, non-compatible, di-mon matings, e.g., $(A^1B^1 + A^2B^2) \times A^1B^3$, in which neither nuclear type of the dikaryon is compatible with that of the homokaryon.

The biological significance of dikaryotization of homokaryons by dikaryons as readily as by compatible homokaryons, as pointed out by Buller (1931, 1941), lies in the greater extension in a limited substrate of a unified mycelial system leading to the production of the maximal number of spores via sexual recombination.

Quite aside from the significant role which this mode of dikaryotization may have in nature, di-mon matings are of particular interest because of their usefulness in the experimental study of certain basic aspects of tetrapolarity. Quintanilha (1939), working with *Coprinus fimetarius*, utilizing compatible di-mon matings and determining the mating-type factors carried in the f_1 progeny, was able to demonstrate a lack of equivalence between the members of a series of four distinct *A* factors. In a number of matings involving the *A* factors, *A*, A_1, *a*, and a_1[8], in all dikaryon vs. homokaryon combinations, with appropriate *B* factors, certain *A* factor combinations were preferentially established in the newly dikaryotized mycelium, e.g., $(a_1\text{-} + A_1\text{-}) \times A\text{-} \rightarrow Aa_1$ rather than AA_1; $(a_1\text{-} + A_1\text{-}) \times a \rightarrow aa_1$, rather than A_1a, etc. Summation of his results revealed that the different *A* factors became associated as though they formed a linear series,

——*a*——*A*——A_1——a_1——[8]

and in each instance, of the two compatible types in the dikaryon, that one which was more distant in the series from the type of the homokaryon was found to be the dikaryotizing agent.

A morphological distinction between legitimate and illegitimate di-mon matings was originally recognized by Buller. The illegitimately dikaryotized mycelium displayed an irregular pattern of dikaryotization and of subsequent growth, which he termed "patchy," whereas the legitimate reaction produced dikaryotic mycelium of normal appearance. The illegitimate or non-compatible dikaryotization of homokaryons has intrigued many workers, of whom several have advanced hypotheses to account for the phenomenon. The more important postulations are: (1) *Both* nuclear types of the dikaryon may migrate across hyphal bridges and eventually replace the original nuclear type. This hypothesis was formulated by Rawitscher (1933) and favored by Buller (1933, 1941), Chow (1934), Dickson (1934, 1936), and Oikawa (1939) with little experimental basis beyond Dickson's evidence of double migration in a legitimate di-mon mating of *Coprinus macrorhizus*. (2) In the dikaryotic mycelium during conjugate division, an exchange of nuclear material, including an incompatibility factor or probably an entire chromosome, might constitute a new nuclear type, which being compatible with the homokaryon, would serve as the dikaryotizing agent, e.g., $(A^1A^1 + A^2B^2) \rightarrow A^1B^2$ or A^2B^1. This hypothesis of Quintanilha's (1938) was declared inadequate by Buller (1941) because of its failure to explain the "patchy" habit. (3) The mutation of an incompatibility factor, either at the *A* or *B* locus, would produce a combination having no factor in common with the homokaryon.

In *Schizophyllum commune* the distinction between legitimate and illegitimate di-mon matings is very striking, and Papazian (1950c) made use of the distinction to develop a method of determining the constituents of dikaryons by means of vegetative matings, thus avoiding Quintanilha's laborious method, which required the production of fruits and the analysis of basidiospores. For any given di-mon mating, the number of different probable dikaryotic combinations is small, e.g., in the mating, $(A^1B^1 + A^2B^2) \times A^1B^2$, the probable

dikaryons, on the basis of the hypotheses so far advanced, would be: ($A^1B^1 + A^2B^2$), ($A^1B^2 + A^2B^1$), ($A^1B^2 + a^2B^2$), and ($A^1B^2 + A^2b^2$). These dikaryons may be differentiated according to the pattern of hemi-compatible vs. non-compatible reactions with a number of tester strains:

	A^1B^1	A^1B^2	A^2B^1	A^2B^2
($A^1B^1 + A^2B^2$)	C*	N*	N	C
($A^1B^2 + A^2B^1$)	N	C	C	N
($A^1B^2 + a^2B^1$)	N	C	C	C
($A^1B^2 + A^2b^2$)	C	C	C	N

* C—Hemi-compatible, N—non-compatible.

Any series of possible dikaryons can be differentiated by this method with the proper tester homokaryons.

Using this method of determination of dikaryotic constituents, Papazian analyzed numerous non-compatible and compatible di-mon contrasts in an attempt to clarify the mechanism of illegitimate dikaryotization and to confirm the observations of Quintanilha on the non-equivalence of incompatibility factors.

Thirty-nine non-compatible di-mon matings, representing 13 different factor combinations, were made; of these, 20 matings of 12 different kinds yielded dikaryons. Sixteen dikaryons of 9 different kinds were tested. In 6 of 8 unambiguous cases, represented by a total of 14 matings, both nuclear types of the original dikaryon were recovered from the new dikaryon, while in the 2 remaining, a new nuclear type was found as the dikaryotizing agent. In the two latter cases, a total of 6 replicates, 4 and 2, were tested. Therefore, instead of resolving the controversy between Rawitscher, Buller, et al., and Quintanilha by proving the hypothesis of one or the other to be correct and exclusive, these findings proved both to be correct but non-exclusive.

More recent work by Papazian (pers. com.) along the same lines, but using a marker linked to the *A* factor, has demonstrated the integrity of a considerable chromosomal segment during reconstitution, a result which strongly supports the hypothesis of at least occasional chromosomal exchange during conjugate division. A sufficient number of genetic markers, biochemical and morphological mutants in linkage groups with both *A* and *B* factors, are now available in *S. commune* to resolve any lingering doubts about the matter.

The probable cause of the characteristic "patchy" habit of illegitimate matings was also revealed by Papazian's work. This concerns heterokaryosis, another aspect of tetrapolarity which will be discussed in the following section.

In seeking to extend the results of Quintanilha on the nonequivalence of incompatibility factors to *S. commune*, Papazian (1950c) determined the constitution of newly formed dikaryons in a number of compatible di-mon matings. That his efforts produced totally ambiguous results in respect to the primary objective was mitigated by unexpected findings of perhaps equal significance. Among 19 different matings, the expected dikaryotization by one or the other of the nuclear types of the dikaryon occurred in only 11 cases. In 2 of the remaining 8 cases, both nuclear types of the dikaryon migrated into the homokaryon and displaced the original nucleus, while in 6 different matings, new nuclear types were formed by recombining factors from both members of the dikaryon. No hypothesis to account for either of these seemingly useless patterns of nuclear behavior was advanced.

It is thus apparent that the ramifications of the "Buller Phenomenon," or di-mon matings, are numerous, and that the phenomenon well serves both the plant and the investigator.

5. *De-dikaryotization ("De-diploidisation" of Buller)*

It has been demonstrated in a number of tetrapolar species which produce asexual reproductive spores that the oidia or conidia produced by the dikaryotic mycelium may be typically uninucleate.

This end-result may be achieved in different ways in various species. (1) In *Pholiota aurivella*, Vandendries and Martens (1932) reported the entry of a dikaryotic pair of nuclei into the developing, elongated ooidium, which in its further growth became septate to form two uninucleate cells. The two cells of the spore frequently break apart, each germinating to produce a homokaryotic mycelium. (2) In *Collybia velutipes*, two types of oidiophores and oidia were shown by Brodie (1936b) to be produced, dikaryotic and monokaryotic. In the latter type, a single member of the dikaryotic pair moved into the oidiophore and there, by repeated division, provided single nuclei for a series of uninucleate oidia. (3) In *Peniophora allescherii* and *Corticium effuscatum*, Nobles (1935, 1942) found yet another mode of de-dikaryotization. Into each developing conidiophore of these species there migrates a conjugate

pair of nuclei which, by repeated divisions, produces several daughters, one of which migrates into each conidium. From a single conidial head, therefore, about one-half of the conidia are derived from each member of the dikaryotic pair.

Monokaryotic oidia and conidia produced by dikaryotic mycelia may function either to reestablish the two component homokaryons in new localities, thus performing a strictly asexual role, or to serve as dikaryotizing agents. The incidence of germination of oidia and conidia to form homokaryotic mycelia varies in different species, and it is probably in the capacity of dikaryotizing agents that these monokaryotic derivatives of dikaryotic mycelia achieve their more important role, particularly when it is recalled, as pointed out by Brodie, that many of these forms or the substrates upon which they grow are attractive to insects which serve as the ideal dispersal agents.

De-dikaryotization of forms lacking oidia or conidia has been accomplished by microsurgery of dividing terminal cells in a number of cases in connection with experimental studies (Harder, 1926, 1927; Papazian, pers. com.). Homokaryons can easily be recovered from dikaryotic combinations, a useful laboratory procedure on occasion, by macerating dikaryotic mycelium in a Waring Blendor, streaking the macerate thinly upon semisolid medium, and isolating young, growing hyphae lacking clamp connections (Raper and San Antonio, unpub.).

VII. HETEROKARYOSIS

A second type of mycelial interaction, very different from but paralleling in many respects the process of dikaryotization, occurs in matings which bring together homokaryons having *A* factors in common and, to a much lesser degree, *B* factors in common. In the former case, reciprocal migration of nuclei rapidly converts both original mates into a single heterokaryotic mycelium; while in the latter case, a limited heterokaryotic mycelium, probably likewise reciprocally constituted, is formed along the line of contact of the two contrasted mycelia. These heterokaryons differ strikingly from homokaryotic and dikaryotic mycelia and deserve brief description as an integral part of the tetrapolar complex. The following description is based largely upon Papazian's account of *Schizophyllum commune* (Papazian, 1950c), but includes certain observations of the writer on this and other species.

Common-A heterokaryon, "Flat"

E.g., $A^1B^1 \times A^1B^2$. The common-*A* heterokaryon, as compared with a normal homokaryotic or dikaryotic mycelium, exhibits a sparse, depressed growth, having few if any aerial hyphae. The linear rate of growth is normal to markedly subnormal. The heterokaryon is completely stable, under ordinary conditions of growth and *when constituted by two normal homokaryons*, having been subcultured for many transfers without evidence of sectoring into its components. Microscopically, it is composed of varying proportions of (a) large, radial hyphae which are gnarled and knobby, and often have pseudoclamps or even rarely true clamp connections at some septa; and (b) small, smooth, irregular hyphae which are randomly oriented. The percentage of hyphae of type *a* rarely exceeds 20 per cent. As observed by phase microscopy, most cells have a single nucleus each, a very few have two nuclei, which, however, are not associated as are those of the dikaryon, and fewer still are apparently enucleate. Isolated hyphal tips, for the most part, yield normal homokaryotic mycelia, a reasonable finding in view of the prevalence of uninucleate cells. The heterokaryon is sterile, producing neither fruits nor any stage beyond itself. It can, however, furnish dikaryotizing nuclei to any homokaryon with which either of its component nuclear types is compatible, e.g., in intrastock matings, $(A^1B^1 \times A^1B^2) \times A^2B^2$, $A^2B^1 \rightarrow (A^1B^1 + A^2B^2)$, $(A^1B^2 + A^2B^1)$, and in all interstock matings. Its role in dikaryotization is unilateral; it cannot become dikaryotized, and attempts to introduce into it a third nuclear type have been unsuccessful. It readily heterokaryotizes appropriate homokaryons, e.g., $(A^1B^2 \times A^1B^1) \times A^1B^1$, $A^1B^2 \rightarrow (A^1B^2 \times A^1B^1)$. Metabolically, it differs markedly from normal homokaryotic and dikaryotic mycelia in response to various carbohydrates and other nutrients (Roshal, unpub.), in acid production, in glucose utilization, and in its rate of growth (Raper and San Antonio, in press.).

The exact nature of the end-product of the interaction between common-*A* homokaryons presents something of a puzzle. Because of its stability, distinct morphological characteristics, constancy of physiological behavior, and dual mating response, Papazian considered the "flat" myce-

lium a reciprocally constituted heterokaryon. A limited number of hyphal tip isolations, however, yielded ambiguous results: of 20 isolated tips, 19 produced normal homokaryons of one or the other component nuclear types, while 1 produced a "flat"-like mycelium which exhibited a homokaryotic mating response (Papazian, 1950c). Papazian's interpretation was discounted by Jinks (1952), who, defining heterokaryosis as "the association in all cells of a lineage of nuclei of unlike genetic constitution," emphasized the failure to demonstrate nuclei of two distinct types in individual hyphae. Although the original definition of heterokaryosis specified an intracellular association of unlike nuclei (Hansen and Smith, 1932), a somewhat less rigorous definition, *the occurrence within a single mycelium of nuclei of unlike genetic constitution*, is now commonly accepted and is necessary to provide sufficient flexibility to encompass both the great numerical disparity of the two nuclear types frequently encountered in balanced heterokaryons of *Neurospora*, etc. (Beadle and Coonradt, 1944; Gross, 1950), and the occurrence of typically uninucleate cells, viz., sterigmata, in the heterokaryons of certain species of *Penicillium*, etc. (Rees and Jinks, 1952).

A more adequate characterization of the common-*A* heterokaryon of *Schizophyllum commune* and other tetrapolar species constitutes one objective of current work in this laboratory. Although the characterization of the "flat" mycelium is still incomplete, two significant findings have been clearly established.

(a) True heterokaryosis occurs in individual hyphae of the "flat" mycelium. The results to date, however, do not preclude the presence within the heterokaryon of sizable elements which remain homokaryotic indefinitely. Hyphal tips, comprising one and two cells, are uniformly homokaryotic; the presence of two types of nuclei has been demonstrated, moreover, in a very low percentage of isolated, larger tips of 10 to 50 cells. Heterokaryosis appears to be restricted, under normal conditions of growth, to the older portion of each hyphal system.

(b) The "flat" heterokaryon differs from heterokaryons of *Neurospora*, *Aspergillus*, *Penicillium*, and Fungi Imperfecti, etc., in that it does not constitute a system of internally mutual complementation. Factors limiting the growth of either nuclear component alone cause an inhibition of growth of the heterokaryon per se and is usually followed by the liberation, through sectoring, of the unaffected homokaryon. Also, heterokaryons constituted of two biochemically deficient homokaryons cannot grow upon minimal medium.

These results have been obtained in the course of three lines of experimentation: (1) hyphal tip isolations and the subsequent characterization of the resultant mycelia; (2) comparison of behavior, i.e., growth, morphology, and sectoring, of heterokaryons containing morphological *and* biochemical mutants on complete, minimal, and supplemented media; and (3) the sequence of events following the growth of hyphal tips of heterokaryons containing morphological *and* biochemical mutants from complete medium into a medium in which only the morphological mutant can grow.

These studies are reported in detail elsewhere (Raper and San Antonio, in press).

Common-B heterokaryon, "Barrage"

E.g., ($A^1B^1 \times A^2B^1$). The common-*B* heterokaryon has only been demonstrated in *S. commune*, by Papazian (1950c), and differs from the common-*A* heterokaryon, aside from its specific origin, primarily in its limited extent and lack of stability. On the few occasions on which it was successfully isolated from the "barrage" region of common-*B* matings, it produced numerous sectors of purely homokaryotic mycelium.

Nuclear constituents of both types of heterokaryons, the "flat" and the "barrage," can be identified in respect to incompatibility factors by mating with appropriate tester strains.

"Flat" heterokaryotization of homokaryons occurs in various types of contrasts which bring together nuclear types with common *A* factors: homokaryon × homokaryon, "flat" heterokaryon × homokaryon, and dikaryon × homokaryon—provided in all cases that no compatible combination exists between the homokaryon and the components of the compounded mate. Heterokaryotization is thus always a second choice to dikaryotization, occurring only when the latter association is not *immediately* possible.

Emphasis on the word "immediately" in the preceding sentence recalls the illegitimate dimon matings of Buller and the "patchiness" which the homokaryotic mycelia display in matings of this type. Analysis of *all* of the nuclear associations within an illegitimately dikaryotized homokaryon has not been attempted, but there can be

little doubt what such an analysis would reveal: one or more specific dikaryons in the isolated spots of normally appearing mycelium, common-*A* heterokaryon in the relatively large expanses of depressed growth, and, probably common-*B* heterokaryon along the line of contact between the two mycelia. The various possibilities may be summed as follows:

$$(A^1B^1 + A^2B^2) \times A^1B^2 \rightarrow \begin{cases} (A^1B^2 + A^2B^1) & \text{Dikaryon} \\ (A^1B^1 + A^2B^2) & \text{Dikaryon} \\ (A^1B^1 \times A^1B^2) & \text{"Flat"} \\ (A^2B^2 \times A^1B^2) & \text{"Barrage"} \end{cases}$$

A^2B^1

The establishment of either type of heterokaryon in a given region blocks at its boundary the further passage of dikaryotizing nuclei, singly or in pairs. The typical "patchy" appearance of illegitimate di-mon matings, in those cases in which new nuclear types are reconstituted, would indicate that recombination within the dikaryon is a relatively rare event and that dikaryotization through this agency is subject to appreciable delay, while in those cases in which the dikaryon takes over, the migration is hampered by localized heterokaryosis.

Rationalization of the biological utility of these sterile heterokaryons is difficult. They constitute, as it were, a negation of the process of dikaryotization, since heterokaryotization of any homokaryon removes it permanently from the potential dikaryotic complex. Thus heterokaryosis would appear to be of negative value to the species; and a very important consequence of heterokaryosis, to be discussed in the following section (VIII), adds considerable weight to this evaluation. In nature, however, heterokaryosis is probably not very common in species having a very efficient spore dispersal mechanism, since the frequency of two mycelia having common factors growing together must be very low. It might, however, play a very important role in isolated cases in which dispersal of spores is hindered for some reason or other.

VIII. MUTAGENESIS, A CONSEQUENCE OF HETEROKARYOSIS

This section was presented substantially as given here at the meeting of the Genetics Society of America at Ithaca, N. Y., September 1952, under the title, "Heterokaryotic Mutagenesis in the Tetrapolar Fungus, *Schizophyllum commune*," (Raper and San Antonio, 1952).

In aging cultures of the "flat" heterokaryon of *Schizophyllum commune* there appear, beginning as early as 5 to 7 days after inoculation, small, dense growths, and by the end of a month's incubation the number of these may reach a hundred or more per standard, 90 mm. culture. This phenomenon was first observed by Papazian (1950c), who isolated a number of such atypical inclusions and showed them to be homokaryotic. He characterized in detail and made a genetic study of one, "streak," which he found to be a single gene mutant at a locus linked to the *A* incompatibility factor (1951). At least one other mutant type was reported by Papazian, but it was not studied in detail.

For the past three years, the phenomenon of *heterokaryotic mutagenesis*, a term coined by the writer (Raper and San Antonio, 1952), has been the subject of an intensive investigation in this laboratory. The following account, drawn partly from Papazian's original work and publications but predominantly from our own investigations, constitutes a summary of findings which will be presented in detail in a series of forthcoming papers.

The isolation, culture, and determination of mating type of about two thousand individual atypical growths appearing in common-*A* heterokaryons permit their characterization as follows:

(1) Each is homokaryotic.

(2) Each exhibits a specific morphological aberrancy, of which there are several distinct types varying from only slightly different from normal to extreme aberrancy. In the stock most intensively studied, Papazian's $A^1A^2B^1B^2$, eight types have been characterized and each has been designated according to its outstanding morphological feature: "streak," "puff," "fluff," "thin," "feather," "fir," "dwarf," and "vesicular." A few other, more rarely occurring types have not yet been studied in detail.

(3) Each type so far analyzed differs from wild type by a single mutated locus, which is both characteristic and distinct for each morphological type.

(4) Each mutant allele is recessive to its wild-type counterpart. Either dikaryotic or heterokaryotic mycelia carrying a mutant strain as one of its components are indistinguishable from corresponding mycelia carrying two normal components.

Three additional characteristics typically apply to heterokaryotic mutants, but occasional exceptions to their universal applicability have been observed.

(1) Most of the heterokaryotic mutants mate unilaterally, i.e., each provides dikaryotizing nuclei for any normal compatible mycelium but ordinarily does not itself become dikaryotized. "Puff" and a number of pigmented mutants from a different stock are occasional exceptions to this rule. The character of unilateral mating segregates with the morphological features of the various mutants showing this trait, and is therefore considered as a direct expression of each of the several affected loci.

(2) Most of the heterokaryotic mutants are quite stable in continued culture for prolonged periods. Three types, "feather," "vesicular," and "puff," however, frequently produce sectors which appear, in growth rate and in morphology, to be intermediate between the parental mutant and normal. Partial back-mutation was suspected, but analysis of two of these revealed instead that mutations at at least two additional loci had occurred. For example, a mating between a fast-growing sector of "vesicular" and wild type yielded, in addition to normal and unmodified "vesicular," a number of classes in which the normal and "vesicular" habits were variously modified.

(3) Heterokaryotic mutants are typically distinct from mutants of the same strains induced by irradiation with ultraviolet. Biochemical and morphological mutants in large number have been isolated from irradiated homokaryons and characterized, but only two single individual mutants of the heterokaryotic types, one "puff" and one "streak," have been recovered following irradiation. Otherwise the two groups are distinct. Conversely, many heterokaryotic mutants and many normally appearing homokaryons isolated from mutagenically active heterokaryons have been tested for biochemical deficiencies. Each grew normally upon the minimal medium.

Studies directed toward elucidating the process of heterokaryotic mutagenesis itself have naturally been of primary interest in the investigation. Progress in working out the details of a process originating from as complicated a system as heterokaryosis is expectedly slow, but a number of preliminary findings are of interest and significance.

(1) According to the best estimates so far possible, mutations occur in heterokaryons in frequencies ranging from 10^{-6} to 10^{-9} per nucleus of mature cultures as compared to a maximum of 10^{-11} for the spontaneous occurrence of these mutants in the component homokaryotic strains. During three years only a very few have been found in an estimated 3000 homokaryotic cultures, including many controls which were subjected to various experimental indignities along with the heterokaryons. Contamination has not been excluded in the few cases in which "mutants" have been found in homokaryotic cultures. It is of interest, however, that Roshal (unpub.) reports the regular occurrences of mutants of heterokaryotic types in homokaryotic cultures to which new nutrient was added at intervals over a prolonged period.

(2) A definite correlation exists between the morphological type of the mutant and the time of appearance of the mutant in the heterokaryotic mycelium. Those which appear during the first two weeks are predominantly of the less aberrant types, "streak," "fluff," and "thin," while those which appear later are predominantly of the more aberrant types, "fir," "feather," "vesicular," etc.

(3) Each stock produces its own characteristic "spectrum" of mutants. The mutants produced by heterokaryons of three different stocks, collected in Illinois, Wisconsin, and California, included a few types common to all, e.g., "streak," "thin," and "feather," and others peculiar to a single stock, e.g., the California stock yielded many mutants which produced an intense blue pigment not seen in the others. Furthermore, corresponding types in the three stocks showed minor but distinct morphological differences, e.g., certain mutants of each stock were easily recognizable as "streak," but they are not identical, as are the "streaks" of any single stock.

(4) Different heterokaryons differ tremendously in their abilities to induce the characteristic mutants. Comparison of mutagenesis in numerous heterokaryons constituted among the f_4 and f_6 strains of an isogenic series revealed a mutation frequency of only 0.5 per cent that in heterokaryons between the same strains and non-isogenic mates. Attempts to breed high-mutant-yielding strains have so far been unsuccessful; the converse, rather, as been effected. At the moment two facts appear to be established: (1) mutagenesis is not due to a double dose of the *A* incompatibility factor, and (2) mutagenesis is not a single gene

effect. It is of interest in connection with the former conclusion that mutants, of the same types as in common-*A* heterokaryons but in lower frequency, have been recovered from mixed cultures of common-*B* homokaryons.

(5) Preliminary experiments have demonstrated that the capacity for mutagenesis is transferable in the filtrate of aged heterokaryons. Numerous mutations, of several types previously isolated directly from heterokaryons, have been recovered from normal homokaryotic mycelia following their growth for two weeks or longer in the cell-free filtrates of common-*A* heterokaryons which had been incubated in "stand-culture" for 12 to 16 weeks. Positive and significant results have been repeatedly obtained, but the experimental procedure has not yet been sufficiently refined to yield quantitatively satisfactory and predictable results. The failure readily to obtain reproducible results in a system of such complexity is hardly surprising. It is hoped that these difficulties can be satisfactorily resolved through an intensive study of the mutagenicity of filtrates of heterokaryons which is currently in progress.

The phenomenon of heterokaryotic mutagenesis, still but partially elucidated, constitutes a type of auto-mutagenesis so far unknown elsewhere. It appears to operate through the normal production by the heterokaryon of a mutagenic agent (or agents) which preferentially affects a small number of loci in a regular and predictable manner. It is, of course a logical suspicion that the observed mutations in the heterokaryon result from spontaneous mutations at random loci under selective conditions which favor the expression of a few types. Several lines of evidence which indicate a more positive causality of mutagenesis than selection, however, are implicit in certain of the characteristics of heterokaryons and in the apparent mode of origin of the heterokaryotic mutants. The summed implications of (a) the incidence of mutations under different conditions, (b) the mode of expression of mutations in heterokaryons, (c) the different "spectra" of heterokaryotic mutants and mutants induced by ultraviolet light, (d) the variability of mutagenic capacity of heterokaryons having different genetic histories, (e) the correlation between age of heterokaryon and mutant type, etc., constitute strongly indicative evidence for a specific mutagenic mechanism. These lines of evidence will be fully developed in a forthcoming paper. It must be admitted, however, that the certain distinction between the two possibilities is difficult and that the available evidence can be considered neither final nor conclusive.

As pointed out earlier (cf. section V), heterokaryosis between mycelia having common-*A* factors has been demonstrated in a number of tetrapolar species besides *Schizophyllum commune*. Atypical inclusions which appear to be mutants, by comparison with those of *Schizophyllum*, have been seen in heterokaryons of several species, and numerous aberrant mycelia have been isolated from the common-*A* heterokaryons of one species, a member of the Thelephoraceae. These results, though very fragmentary as yet, indicate heterokaryosis and heterokaryotic mutagenesis as probably common attributes of tetrapolar sexuality.

IX. DISCUSSION

Due to the voluminous pioneering works of Kniep and the equally extensive investigations of Buller, Vandendries, Quintanilha, and many of the students and associates of these men, the sexuality of the Hymenomycetes is more fully elucidated than that of any other group of fungi. In addition to homothallism, the patterns of sexuality found here—bipolarity and tetrapolarity involving multiple incompatibility factors—have been found among fungi only in the related groups, the Gasteromycetes and the "Jelly Fungi."

Many species of the Ustilaginales, the smuts, have been described as tetrapolar, complete with multiple incompatibility factors. In a recent reevaluation of the available data on sexuality in this group, Whitehouse (1951), however, reaches the conclusions (1) that neither tetrapolarity nor multiple incompatibility factors has been demonstrated unambiguously in any species, (2) that extra-incompatibility-factor modifiers of mating behavior, etc., frequenty have been misinterpreted as a multiple factor system, and (3) that obligatory cross-mating in all heterothallic species probably depends solely upon a single pair of alleles at a single locus. Sexuality in the smuts, according to this interpretation, would thus correspond more closely to that of the rusts than to that of the Hymenomycetes.

In the former group tetrapolarity has been reported in a number of species (Brodie, 1948; Fries, 1940, 1943), while in the latter bipolarity has been demonstrated in several species (Barnett, 1937). Comparable mechanisms are known in a few other groups of organisms, such as the restriction of fertilization by incompatibility factors in the

flowering plants (Lewis, 1949). In the case of flowering plants, the restriction by incompatibility is superimposed upon a highly evolved sexual mechanism, whereas in the fungi this type of regulation either has replaced a strictly sexual mode of differentiation or has become established in the absence of sexual differentiation.

Two conclusions of evolutionary significance are indicated by the distribution of tetrapolarity, bipolarity, and homothallism in the Hymenomycetes: (1) that control of mating behavior by multiple incompatibility is of very ancient origin; and (2) that the specific distribution of the different patterns of sexuality is of recent origin and is probably the result of a continuous process of change from tetrapolarity to bipolarity to homothallism (Raper, in press). The former conclusion follows from the occurrence, throughout a large group representing may distinct and divergent phylogenetic lines, of a highly evolved differentiating mechanism which is absolutely unique to this group of organisms. The latter conclusion follows from the occurrence, within each of many subgroups at the level of the genus, of all three specific patterns to be found in the whole of the larger group. The genus *Coprinus*, a recently evolved and phylogenetically unified group by all reasonable criteria, illustrates the latter point.

Tetrapolar sexuality constitutes a fairly complex system, which affords, even when the frequent anomalies are disregarded, a number of distinct but interrelated facets. Many of these, puzzling when first encountered, have been satisfactorily explained and integrated into the over-all mechanism; others are but partially elucidated, although in some cases the concepts, techniques, and/or materials are currently available for their final solution. Still others are as yet known only in the descriptive stage, and for these both the responsible mechanisms and the biological significance are still to be determined. A number of points, bearing on the less known aspects, deserve brief discussion.

The multiple incompatibility factor series at each of two "loci" has long been considered one of the more significant features of tetrapolarity, constituting the most efficient mechanism known to facilitate outbreeding while restricting inbreeding to a minimal level. Papazian's preliminary results on intra-factor crossing-over in a relatively small sample (300) strongly indicates that a minimum of four loci comprises the *A* incompatibility factor in a representative tetrapolar species, *Schizophyllum commune*. The possibility that no true multiple allelism is involved here is by no means a remote one, and the analysis of a somewhat larger sample, of the order of 1000 individuals, would almost certainly resolve the genetic structure of the factor.

Only a single attempt has been made to confirm the non-equivalence of members of the multiple incompatibility factor series reported by Quintanilha. This attempt of Papazian gave ambiguous results, and it was concluded, for the compatible di-mon matings with which he worked (cf. Sect. VI), that "the choice of [dikaryotizing] nuclei appears to be definitely governed by some factor other than the alleles of either of the incompatibility factors." This author pointed out the desirability of the use of isogenic strains to avoid extra-incompatibility factor effects. To go one step further and to use only isogenic strains of a single stock, within which intra-factor recombination resulting from crossing-over would provide the range of factors required, would constitute an additional meaningful refinement of the original experimental procedure used by Quintanilha and could well yield results pertaining to the specific genetic mechanism of incompatibility control. Such an approach, aimed directly at the physiological differentiation of incompatibility factors as affected by the specific relationships of their component genes, would appear to be one of the more promising lines for future experimental work on basic aspects of tetrapolarity.

The illegitimate dikaryotization of homokaryons by dikaryons has been particularly intriguing since its first demonstration twenty years ago. One aspect of such reactions, the distinctive pattern termed "patchy" by Buller, is subject to rationalization through the recently discovered phenomenon of heterokaryosis. Whether this postulated explanation is the correct one, however, can only be determined by analysis of nuclear associations from various portions of the irregularly dikaryotized mycelium. Another intriguing aspect of these illegitimate matings is the mode and locale of origin of the newly constituted nuclei from the two nuclear components of the dikaryon. The demonstration that such reconstitution does indeed occur in certain cases gives no indication of how it is accomplished. Chromosomal exchange, as suggested by Quintanilha, seems totally at variance both with an otherwise orderly conjugate

division and also with the fact that in most fungi, if not all, nuclear division occurs within the nuclear membrane (Bagchee, 1925; Harper, 1905; Kniep, 1915). A possible explanation might lie in a process comparable to that demonstrated by Pontecorvo and Roper (1952; also Roper and Pontecorvo, 1952) in heterokaryons of certain ascomycetes which carried complementary biochemical deficiencies and in which treatment with camphor induced the formation of heterozygous diploid nuclei which gave rise to diploids homozygous for new genetic combinations. Such rearrangements might occur regularly but rarely within the normal dikaryotic mycelium, or they might be favored in boundary regions where heterokaryosis has produced abnormal environmental conditions. That needlessly reconstituted nuclear types are common in compatible di-mon matings indicates their regular occurrence in dikaryotic mycelia. Again, these problems are resolvable with techniques and materials which are currently available.

Other aspects of tetrapolar sexuality appear, in the dim light of current knowledge about them, as not only irrational but as positively detrimental to the species exhibiting them. Chief among these, of course, are the interrelated phenomena of heterokaryosis and heterokaryotic mutagenesis. In any consideration of heterokaryosis in these forms, a striking difference between heterokaryons here and in the Ascomycetes requires emphasis. In the heterothallic ascomycetes, heterokaryons are most readily constituted between individuals of the same mating type, only with difficulty between the two mating types, and by mutual complement tend to overcome their separate and individual inadequacies. In *Schizophyllum commune*, on the other hand, heterokaryons form only between mycelia of unlike strains and here the mutual interaction is anything but complementary. Papazian, using his "streak" mutant as a marker, reported unsuccessful attempts to demonstrate heterokaryosis between mycelia of the same strain, and the writer, employing biochemically deficient mutants and highly selective methods of recovery, has similarly failed to detect any nuclear exchange. Heterokaryosis here would thus appear to serve different purposes than those generally ascribed to heterokaryons. The biological advantages to the species, if any, must therefore in some manner be implicit in the tremendous increase in mutations which results from heterokaryosis.

Any benefits accruing through the increase of the particular mutants which have been shown to occur, however, are difficult to envision. Of about two thousand individual mutants isolated and studied in some detail to date, no single one can be considered the peer of the original wild type by any reasonable criteria. The generally depressed character of the heterokaryons and the marked aberrancy of the heterokaryotic mutants may favor the "easy" conclusion that the interrelated phenomena of heterokaryosis and mutagenesis represent a series of biological accidents. There are, however, a number of facts which indicate for them a biologically significant role: (a) the extreme regularity of the processes, (b) their common occurrence among tetrapolar forms as indicated by their demonstrated occurrence in several unrelated species, and (c) their indicated occurrence in several species in nature.

One possibility here should not be overlooked. Heterokaryosis may produce—whether at random or specific loci is of no consequence—a wider "spectrum" of mutants than is currently appreciated, and of these only the types showing spectacular aberrancy are detected by crude, initial techniques. Thus a wealth of less drastic mutants, which in recombination could serve the evolutionary needs of the species, may regularly be produced along with the detrimental ones. Were this the case, the phenomenon of heterokaryotic mutagenesis could thus greatly augment the usual supply of new genetic material available to the species through spontaneous mutation, etc. Thus those nuclear associations which cannot lead to dikaryotization and the sexual process may yet make a significant contribution toward the better ultimate adaptation of the species.

Such, then, is the current status of knowledge of tetrapolar sexuality. This pattern of sexuality, although the most intensively studied of the several distinct patterns to be found among the fungi, still presents many intriguing and puzzling aspects. Certain of these perhaps hold little promise of significance beyond the clarification of specific and minor details of the mechanism of tetrapolarity per se; others unquestionably have broader implications, inasmuch as they relate more or less directly to basic phenomena of universal biological significance. In the latter category, a number of recently discovered manifestations of tetrapolarity, the multi-genic nature of incompatibility factors,

heterokaryosis and the interaction resulting from duplicated genic material of the incompatibility factors, and the process of mutagenesis in heterokaryons, are each currently only partially elucidated. The experimental possibilities afforded by these phenomena, occurring in organisms of which the general characteristics are already well known, denote the utility and versatility of the tetrapolar basidiomycetes as test organisms for the study of a number of basic phenomena, particularly in classic and physiological genetics.

SUMMARY

The basic life cycle of hymenomycetous fungi is defined by the cyclic progression: spore → homokaryotic mycelium—(plasmogamy) → dikaryotic mycelium → sporocarp—(karyogamy—meiosis—) → spore.

Three patterns of sexuality occur in the Hymenomycetes: homothallism, in which the dikaryotic phase is initiated by an intramycelial association of nuclei; and two types of heterothallism, bipolarity and tetrapolarity, in which the dikaryotic phase is initiated by obligatory intermycelial association of compatible nuclei. Self-sterility and cross-fertility are determined in bipolar species by incompatibility factors at one locus, and in tetrapolar species by incompatibility factors at two independent loci.

Tetrapolarity is distinguished by the production of progeny of four distinct mating types by each sporocarp:

$A^1A^2B^1B^2 \rightarrow A^1B^1; A^1B^2, A^2B^1, A^2B^2$.

Fertile matings occur between the different mating types in only those combinations which restore the double heterozygote. Three types of tetrads, the parental 2-type, the non-parental 2-type, and the 4-type, are commonly produced upon basidia of each sporocarp.

An extensive series of alternate factors at each incompatibility locus exists in natural populations of tetrapolar species. The occurrence of unlike incompatibility factors in different stocks, e.g., $A^1A^2B^1B^2$ and $A^3A^4B^3B^4$, allows for complete fertility in interstock matings. A minimal number of the order of 100 alternate factors at each incompatibility locus has been estimated for natural populations.

Occurrence of "new" mating types among sibling monosporous mycelia has long been interpreted as the result of mutations of incompatibility factors. Recent work strongly indicates that the *A* incompatibility factor of *Schizophyllum commune* is composed of a number of distinct genes, between which crossing-over yields new incompatibility factors by genetic recombination. Four to 10 distinct loci are indicated as comprising the *A*-factor. Seven loci, providing 128 distinct combinations, would allow for the estimated multiple factor series in natural populations without necessary recourse to multiple allelomorphs at any single locus.

Mating of homokaryotic mycelia in all intertype combinations yields a series of reactions which exactly reflect specific intermycelial combinations of incompatibility factors:

(a) Unlike *A* factors and unlike *B* factors, $A^1B^1 \times A^2B^2$, permit reciprocal dikaryotization of both participating mates.

(b) Unlike *A* factors and common *B* factors, $A^1B^1 \times A^2B^1$, yield a narrowly limited heterokaryon, termed "barrage," along the line of intermingling of the two contrasted mycelia.

(c) Common *A* factors and unlike *B* factors, $A^1B^1 \times A^1B^2$, yield a reciprocally constituted heterokaryon, termed "flat," throughout both preexisting mycelia.

(d) Common *A* factors and common *B* factors, $A^1B^1 \times A^1B^1$, result in no reaction or in a slight mutual overgrowth of the two mates.

The dikaryotic phase, containing paired compatible nuclei, resembles a diploid phase as regards physiological and genetic characteristics. Dikaryotization of homokaryotic mycelia can occur under a wide variety of circumstances, chief of which are the following: homokaryon × homokaryon, heterokaryon × homokaryon, and dikaryon × homokaryon. The latter, termed the "Buller Phenomenon" or di-mon matings, may be of three types: compatible, $(A^1B^1 + A^2B^2) \times A^3B^3$, and hemi-compatible, $(A^1B^1 + A^2B^2) \times A^2B^2$, both "legitimate" combinations; and non-compatible, $(A^1B^1 + A^2B^2) \times A^1B^2$, an "illegitimate" combination. Dikaryotization of homokaryotic mycelia in non-compatible di-mon matings is accomplished in some cases by the invasion of the established dikaryon and in other cases by means of newly reconstituted nuclei, apparently involving chromosomal exchange at conjugate division in the dikaryotic mycelium.

The common-*A* heterokaryon, "flat," is morphologically and physiologically distinguishable from both homokaryons and dikaryons, is stable in repeated subculture, and provides dikaryotizing

or heterokaryotizing nuclei to homokaryons of appropriate incompatibility factor constitutions. Although the "flat" mycelium is a true heterokaryon, it differs basically from heterokaryons of *Neurospora*, *Penicillium*, etc., in nuclear structure and in its reactions.

A regular consequence of heterokaryosis is the production of homokaryotic, single-gene mutants, of a limited number of types (about 10) and of varying morphological aberrancy, in frequencies as high as 100,000 times the maximal spontaneous rate. This phenomenon has been termed *heterokaryotic mutagenesis*. The "spectrum" of heterokaryotic mutants differs significantly from that of mutants induced by U-V light and is dependent, to a marked degree, upon age and the genetic constitution of the inducing heterokaryon. Heterokaryotic mutagenesis appears to operate through the normal production of a filtrable mutagenic agent, or agents, which preferentially affects a restricted number of loci in a regular and predictable manner.

Certain aspects of tetrapolar sexuality, each intriguing in its own right, offer unique tools for the study of many phenomena of universal biological importance, particularly in the realm of classical and physiological genetics.

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COLLATERAL NERVE REGENERATION

By MAC V. EDDS, JR.

Biology Department, Brown University

"... since in some ways cell biologists are on old ground working with new implements it would be as well if they recognized more fully the antiquity of their sites and looked out for the old forgotten tracks, along which something of value might still be found." (Hughes, 1952a; p. 6)

INTRODUCTION

THE basic problem discussed in this review was apparently first formulated by Exner (1884, 1885) during his study of the innervation of individual muscle fibers in muscles supplied by two nerves. He pointed out that, in such cases, either one nerve neurotizes part of the muscle fibers and the other the rest, or each muscle fiber is doubly innervated. To decide between these alternatives, Exner (1884) experimented with the cricothyroid muscle of the rabbit, a muscle which he first demonstrated to be innervated by the middle as well as the superior laryngeal nerve. Although severance of both nerves led to wasting of the muscle, no muscle fibers atrophied after interruption of either nerve alone. It was therefore concluded that each muscle fiber is innervated by two axons, one from each nerve.

Exner recognized, however, that his conclusion was not consistent with results previously obtained by Gad (1882) in a pioneering study on the innervation of the frog gastrocnemius muscle. After showing that the tension which this muscle developed following stimulation of the entire sciatic nerve was equal to the sum of the tensions developed upon stimulation of individual ventral roots, Gad had denied the occurrence of plurisegmental innervation of its individual muscle fibers. Exner (1885) therefore repeated his experiments on the frog. He transected the largest of the spinal nerves contributing to the sciatic nerve and sacrificed his animals from 5 weeks to 8 months later. After these periods, electrical stimulation of the interrupted spinal nerves proved them to be free of regenerated axons; yet Exner could not demonstrate any consistent change in the weight or the histological appearance of the partly denervated gastrocnemius muscles. Even counts of intact muscle fibers failed to reveal any deficiency which could be attributed to the fractional loss of innervation. Gad's earlier results forced Exner to seek some explanation other than plurisegmental innervation for the maintenance of partially denervated muscle, and he was thus led to the prophetic suggestion that the muscle fibers which were denervated by the original operation became reinnervated by residual nerve fibers of the intact spinal nerves.

Since intensive study of the motor unit was not to begin for 40 years, it is hardly surprising that this suggestion went unnoticed. Even after the formulation of the motor unit concept (Sherrington, 1925; Eccles and Sherrington, 1930), it was generally assumed that the anatomical relation between a neuron and its associated muscle fibers is constant and unchanging. However, Exner's more dynamic view was destined for revival. This revival, as well as its recent consequences, constitutes the subject matter of this review.

RECOVERY OF PARETIC MUSCLES

Modern interest in partial motor denervation developed as a byproduct of clinical attempts to improve the strength of paretic human muscles. Since the nerve to such a muscle retains some of its motor fibers, it was assumed that muscle function would be enhanced if the intact axons could be induced to extend their terminal processes into denervated areas. Early attempts to foster this extension were based on the results of nerve-crossing experiments in which a small proximal nerve stump had been joined with a large distal stump. During regeneration of the former, branching of fibers occurred at the suture line; subsequent

counts of axons indicated a marked distal increase (for early literature, see Aird and Naffziger, 1939; but cf. Weiss and Campbell, 1944; Billig, van Harreveld, and Wiersma, 1946; Romanes, 1946; Thomas and Davenport, 1949). Dogliotti (1935) was apparently the first to propose that the intact axons of a partly deneurotized nerve would expand their peripheral distribution if the nerve were sectioned, sutured, and permitted to regenerate. However, the evidence offered by Dogliotti in support of his suggestion, as well as that subsequently presented by Aird and Naffziger (1939) in a similar study, was neither direct nor wholly beyond criticism.

The first adequate test of Dogliotti's proposal was reported by Hines (1942). In a series of rats, Hines partly deneurotized the sciatic nerve unilaterally by resecting the 5th lumbar nerve (L_5) just proximal to the lumbar plexus (apparently not L_4 as stated; cf. fig. 3 in Hines, Wehrmacher, and Thomson, 1945). In addition, the tibial nerve of the operated side was crushed in several of the animals.

The average degree of partial denervation effected in these cases can only be estimated. In the rat, L_5 is somewhat variable in size but usually contributes at least one-half of the fibers which enter the sciatic nerve (cf. Hoffman, 1952); the remaining fibers are derived mostly from L_4. Fibers from L_5, however, are not equally distributed to the peroneal and tibial musculature. As in other mammals, the tibial nerve obtains its fibers from more caudal levels of the spinal cord than does the peroneal nerve. Thus, the destruction of L_5 in Hines' experiments affected the gastrocnemius muscle much more severely than it did muscles innervated by the peroneal nerve.

In his analyses of the weights of, and the tensions developed by, the gastrocnemius muscles 84 days after operation, Hines demonstrated that muscles whose nerves had been partially deneurotized and then crushed were uniformly inferior to those which had suffered only partial denervation. Since gastrocnemius muscles normally show about 80 to 90 per cent recovery during a similar time interval after simply crushing the tibial nerve, Hines concluded that, although axon bifurcation may occur during nerve regeneration, it has no functional effect on paretic muscles of the rat.

Hines also noted that the partially denervated gastrocnemius muscles still retained an unexpectedly large fraction of their normal weight and strength. On the average, these muscles weighed just over 80 per cent of their contralateral, intact controls and, when stimulated through the tibial nerve, they developed about 66 per cent of the control tension. It was therefore clear that the paretic muscles had weights and strengths quite out of proportion to their fractional loss of innervation. A more extensive analysis (Wehrmacher and Hines, 1945; Hines, Wehrmacher, and Thomson, 1945) subsequently revealed that the tensions developed by paretic gastrocnemius muscles 3 days after resection of L_5 amounted to only about 25 per cent of normal. This value rose to over 40 per cent by 9 days, over 50 per cent by 14 days, and approached 60 per cent by 50 days. Thus, the disproportionately strong contractions encountered earlier in the 84-day cases represent the end result of a gradual recovery process.

In considering the factors which might be responsible for this recovery, Hines, Wehrmacher, and Thomson first ruled out several possible explanations. Regeneration of the severed lumbar nerves, changes in the strength of the control muscles, and activation of denervated areas of the paretic muscles by acetylcholine produced at the intact myoneural junctions, were each evaluated and rejected. Two other explanations were suggested provisionally. First, recovery was attributed to the "hypertrophy of the normally innervated muscle fibers", which by virtue of their increased size would make a larger than normal contribution to the weight and strength of the whole muscle. In view of the rapidity of the recovery process, this hypothesis seemed less attractive than a second, namely, that "the phenomenon may represent a peripheral extension of the processes of viable axones to neighboring denervated muscle fibers or to an unmasking of normally functionless neuromuscular relationships" (Hines, Wehrmacher, and Thomson, 1945, p. 52).

While Hines' studies were in progress, the same problem was under independent investigation by van Harreveld (1945) and by Weiss and Edds (1946). The test object in van Harreveld's study was the rabbit sartorius muscle, which receives its motor innervation from L_5 and L_6 in a ratio of approximately 1:1.5 (average of 10 cases; see van Harreveld's Table 1). Partial denervation was accomplished by completely evulsing L_6 unilaterally. After intervals ranging from 2 weeks to 6 months, comparisons were made of the tensions developed by the paretic and control muscles upon stimulation of the intact 5th lumbar nerves. The results were unequivocal. Two weeks postoperative

stimulation of the muscles through L_5 gave tension values some 1.4 times larger on the experimenta side. After 1, 2, and 6 months, respectively, the experimental tensions averaged 3.6, 4.4, and 6.2 times larger than the control values. Thus L_5, which originally contributed an average of about 40 per cent to the innervation of the sartorius muscle, had not only increased its contribution enough to provide for the innervation of a "whole" normal muscle (reached when the experimental tension is 2.5 times greater than the control), but had actually overshot this value.

The degree of overshooting cannot be inferred directly from the data given. For example, the fact that the experimental muscles after 6 months developed 6.2 times as much tension as the L_5-innervated portion of their controls, is deceptive. The large average value stems primarily from 4 cases with unusually low control tensions. Since a low control tension means a small original participation of L_5 in the innervation of the experimental muscle (cf. Kosman, Wood, and Osborne, 1948), and hence an excessively high degree of partial denervation following the elimination of L_4, the opportunity for expansion of the L_5 contribution was particularly marked in these cases. No data are available to permit a direct comparison, in individual cases, of the experimental tensions after L_5 stimulation with control tensions after stimulation of L_5 and L_6 together. The degree of overshooting can be estimated, however, as of the order of 30 per cent. This value is derived by comparing the average tension developed by normal muscles excited through the entire femoral nerve (147 g., see van Harreveld's Table 1) with the average force produced after 6 months by partially denervated muscles excited through L_6 (192 g., cf. Table 6). Increases of this magnitude are consistent with van Harreveld's statement that, in the older cases, the experimental sartorius muscles "appeared considerably broader and thicker than on the control side." Muscle weights were not recorded.

On the basis of histological evidence, van Harreveld concluded that two changes occur in partially denervated muscles which are responsible for their gradual recovery. First and most important is a growth in the size of the intact motor units; secondarily, the innervated muscle fibers undergo a moderate hypertrophy. His evidence was as follows: (1) in all cases where measurements of tension had indicated marked recovery, cross sections of the partially denervated muscles showed few or no atrophic muscle fibers (cf. Exner, 1884, 1885); (2) the mean cross-sectional areas of muscle fibers on the operated side of older cases averaged some 50 to 60 per cent larger than those of the control fibers. These microscopic findings, plus the previously mentioned inverse relationship between the amount of strength increase exhibited by a paretic muscle and the size of the original L_5 contribution to its innervation, provided cogent evidence that the intact motor units had in fact expanded.

The results reported by Hines, Wehrmacher, and Thomson (1945) and by van Harreveld (1945) were confirmed and extended by Weiss and Edds (1946). Working with the soleus muscle of the rat, they set up paired experiment which ran for from 1 week to 4 months after operation. In one animal of each pair, L_4 and L_5 were unilaterally transected and capped (Edds, 1945) to inhibit regeneration. In the other member of the pair, L_5 was similarly eliminated. On the average, these operations reduced the number of myelinated fibers in the soleus nerve to 80 and 20 per cent, respectively, of their normal values. An analysis of the weight changes of the contractile fractions of the paretic soleus muscles demonstrated that, after a moderate initial atrophy, a steady and progressive recovery ensued, beginning during the second postoperative week. Muscles lacking axons from L_5 underwent a greater relative improvement than did the less affected muscles originally deprived of $L_{4,5}$ fibers and in both instances, near normal weight was recovered by the end of the longest experimental periods. Muscle power likewise improved progressively, but recovery was incomplete even in the oldest cases (cf. van Harreveld, 1945).

The histological findings described by Weiss and Edds were also consistent with the view that the originally denervated muscle fibers had regained nerve connections. The frequency distribution of muscle fiber sizes in the partially denervated muscles was intermediate between those of completely denervated and normal muscles. Six weeks after destruction of L_5, for example, the soleus muscle contained: (a) still denervated, severely atrophied fibers; (b) fibers of normal dimensions which, presumably, had not lost their innervation originally; (c) fibers of intermediate sizes, interpreted as having regained innervation after varying postoperative intervals. Approximately 65 per cent

of the fibers fell in the latter two categories, although only some 20 per cent of the medullated axons remained in the soleus nerve. Since no evidence was obtained for the hypertrophy of muscle fibers with an intact nerve supply (category b), the early functional recovery was attributed solely to motor unit expansion.

Weiss and Edds also noted an abrupt increase in the number of soleus nerve fibers beginning at 8 weeks postoperative. In view of the precautions taken to inhibit regeneration of the severed lumbar nerves, these increases were ascribed to a belated, spontaneous breakdown of residual axons in the sciatic nerves, which subsequently regenerated with some terminal branching. However, reinvestigation (Edds, 1949b) has shown that the late increases in axon number were due to a few fibers which escaped around the plastic caps on the lumbar nerve stumps and reinvaded the test muscles. Thus, the direct intramuscular extension of intact axons alone remained as a possible explanation for the functional and histological observations.

The remarkably consistent picture which emerged from these three studies has since been confirmed in other mammals. Frederick and Kosman (1948) have shown that 5 months after elimination of about 75 per cent of the sciatic nerve fibers in the dog, the anterior tibial muscle develops up to 40 per cent of normal tension and weighs approximately 70 per cent of normal. There are also two studies indicating that monkey muscles improve after moderate, but probably not after severe, partial denervation. Edds and Small (1951) have described the recovery of leg muscles of mature monkeys which had lost from 25 to 50 per cent of their nerve supply. Similar results appear to have been obtained with immature monkeys by Black and Nulsen (1951), although no details concerning the degree of denervation or of recovery are given in their account.

NEUROHISTOLOGICAL STUDIES

However convincing the evidence which had been presented for the adoption of muscle fibers by intact, residual axons, direct proof was lacking. It is true that Nageotte (1906, 1907) and Cajal (1928, p. 412) had recognized that sensory axons can develop collateral branches even in the absence of any detectable traumatic stimulus. Furthermore, the studies of Speidel (1935, 1941) and of Weddell, Guttmann, and Gutmann (1941) on partially denervated cutaneous areas had already demonstrated the extension of sprouts from intact nerve fibers into zones of sensory loss (cf. Livingston, 1947). But these facts merely lent additional indirect support to the hypothesis, and the next step was obviously a neurohistological analysis. This was undertaken independently in two different laboratories (Edds, 1949a, 1950a; Hoffman, 1950).

Both investigators relied mainly on a technique involving gold impregnation (cf. Carey, 1948) which permits the study in teased preparations of relatively large numbers of nerve and associated muscle fibers (Fig. 1). Several muscles of the rat, subjected from 1 to 300 days previously to partial denervation ranging from negligible to almost complete, were examined by this method. Soon after the operation (4 days, Hoffman; during the second week, Edds), delicate collateral sprouts appeared on many of the residual motor axons. These new branches, arising intramuscularly mostly within 200 μ of the end of the parent fiber, attached themselves to adjacent, aneuritic neurilemmal sheaths (derived from interrupted nerve fibers) and followed the latter to denervated end plates (Figs. 1, *b* and *e*). Hoffman also observed other patterns of outgrowth, one of which, the production of ultraterminal branches from intact end plates, likewise resulted in the reinnervation of neighboring muscle fibers. Collateral regeneration was particularly abundant during the third postoperative week, but continued sporadically for several weeks thereafter. Once connected with their adopted end plates, the collaterals began to mature and, early in the second postoperative month, most had attained diameters within the size range of normal fibers (Figs. 1, *c* and *f*).

Quantitative analysis (Edds, 1950*a*) confirmed the suggestions of van Harreveld (1945) and Weiss and Edds (1946) that the number of collateral branches developed in any given region of a paretic muscle would be directly related to the degree of original denervation. Thus, collateral regeneration was far richer in areas of extensive partial denervation where the number of end plates requiring reinnervation was relatively large, than it was in regions where only a few end plates had lost their nerve supply (cf. Fig. 1). Aside from the obvious adaptiveness of this aspect of the process, it bears importantly on the mechanism of sprouting which will be discussed below.

More recently, van Harreveld (1952) has

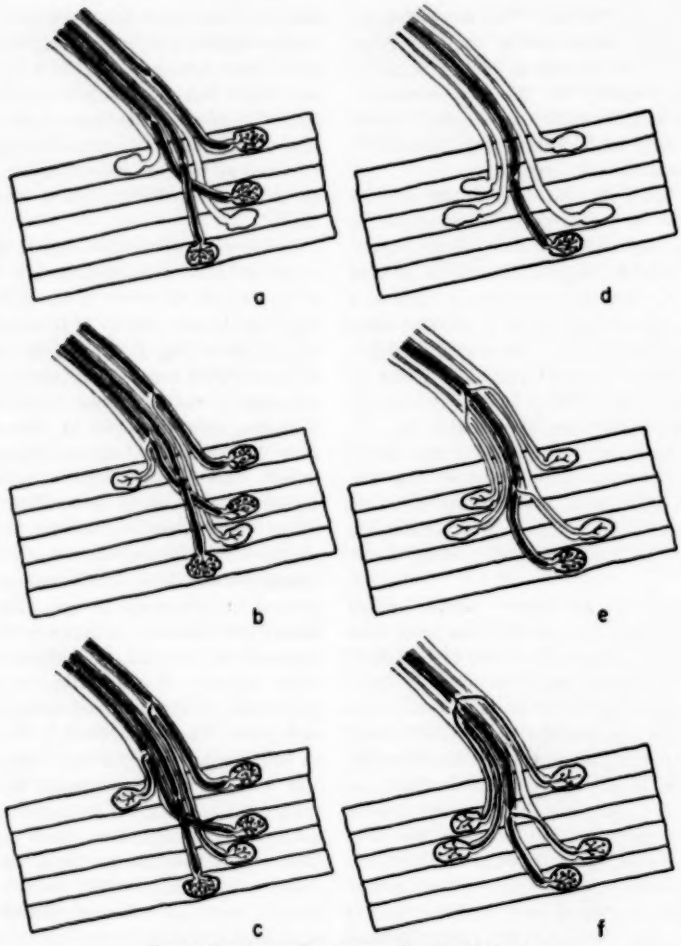

FIG. 1. STAGES OF COLLATERAL REGENERATION

Two terminal bundles of motor nerve fibers undergoing collateral regeneration following partial denervation of their associated muscle fibers. One bundle (*a*) has lost only two fibers; the other (*d*) is severely affected, with only one of its 5 fibers intact. No Schwann cells are shown, but empty neurilemmal tubes mark the original course of degenerated axons. Newly developed collateral branches originate either from old nodes of Ranvier (*b* and *e*) or from an internodal region (*e*). They have penetrated adjacent, empty tubes and reneurotized denervated end plates. In the near-terminal stages (*c* and *f*), the branches have matured; eventually, their calibers approach or equal those of parent fibers. Note in *f* that a new node has developed at the level where three branches arise.

presented confirmatory histological evidence for collateral axon regeneration in paretic rabbit muscles (cf. Morris, 1952). Sartorius muscles which had been deprived of nerve fibers from L_6 at intervals ranging from 3 days to 2 months previously, were studied mostly in silver-impregnated sections. Collaterals (which become more heavily impregnated with silver) from the terminal portions of intact axons were first detected 4 to 5 days after operation, and their formation continued through the first month. Van Harreveld also demonstrated that new branches occasionally arise from more proximal levels of the residual fibers, where they are coursing through large intramuscular branches of the sartorius nerve. Since no collaterals are formed along the extramuscular course of such

nerve fibers (Edds, 1949b), the larger intramuscular nerves seem to constitute a zone of transition in the sprouting capacity of the nerve fiber.

The results obtained in a study of gold-impregnated and teased paretic muscles of the monkey (Edds and Small, 1951) contrast with those just described. Although muscles which had suffered a relatively light denervation were essentially similar to comparable rat muscles, collateral regeneration rarely occurred in muscles which had lost more than half of their original innervation. The reason for this difference is unknown, but its bearing on the recovery of paretic muscles in man is presumably important (cf. Wohlfart, 1949; Bowden, 1951).

MECHANISM OF COLLATERAL REGENERATION

The observations summarized above provide a frame of reference into which must be fitted any hypotheses concerning the causal factors underlying the development of collateral branches and their subsequent connection with denervated end plates. It has been mentioned that, even in its detailed features, the sprouting process is adaptive. Not only is collateral formation confined to the intramuscular portions of the intact nerve fibers, but the number of new branches produced in a given region is in accord with local requirements (cf. Fig. 1). A severely deneurotized spray of nerve fibers is occasionally encountered one or two millimeters from another spray with only a few of its component fibers destroyed. Yet in each spray the amount of collateral regeneration is generally in direct proportion to the number of end plates to be innervated.

In this connection, the tendency of terminally regenerating axons to saturate peripheral tissues should be recalled. Weiss (1937) was the first to emphasize this fact, pointing out that the number of fibers which regenerate into a transplanted larval limb of *Amblystoma* is closely correlated with the size of that limb. When the transplant approached normal dimensions, Weiss found a nearly normal number of nerve fibers present in it; nerve fibers were proportionately less numerous when the transplant was reduced in size. A comparable study by Litwiller (1938) of nerve regeneration in regenerating salamander limbs led to the same general conclusion. Litwiller demonstrated that there is a constant ratio between the number of nerve fibers ending within a given region of a regenerated limb and the volume of that region. In these and similar cases, a balance is presumably established between the capacity of the nerve fiber source to produce new branches and the ability of the periphery to admit those branches, to maintain them, or both. However, the nature of the quantitative controls remains obscure (cf. Weiss and Cummings, 1943; Weiss and Campbell, 1944; Weiss, 1950a).

It is probably more than coincidence that collaterally regenerating axons exhibit a similar tendency to saturate available end plates. A priori, there are three possible explanations: (1) the stimulus for collateral sprouting is well-localized and in proportion to the degree of denervation; (2) the stimulus is diffuse but in various local areas, residual nerve fibers become differentially responsive; (3) both stimulus and response are general; sprouting occurs with equal frequency throughout the muscle but only those sprouts are retained which connect with muscle fibers.

The stimulus

Three different sources of the stimulus have been suggested: denervated muscle fibers; disintegrating myelin of interrupted nerve fibers; sheath cells of degenerating nerve fibers.

Denervated muscle fibers. Van Harreveld (1947) contended that a humoral agent produced by denervated muscle tissue constitutes the stimulus for collateral regeneration. He reported that, during the first 2 weeks, the paretic sartorius muscle recovered strength more rapidly if it were injected with ether extracts of denervated muscle or if the adjacent quadriceps muscle were completely denervated. Negative results were obtained in control experiments with injections of normal muscle extracts. Denervation of more distant muscles supplied by the sciatic ne ve was also ineffective, indicating that the active principle had diffused directly from the quadriceps to the sartorius muscle.

Disintegrating myelin. Despite the use of large numbers of animals, van Harreveld was unable to demonstrate statistical significance for his results. His conclusion therefore remained contestable until supporting evidence, at least for his general concept of a humoral agent, was obtained by Hoffman (1950). Hoffman's test object was the normal anterior tibial muscle of the rat. Following intramuscular injections of a variety of materials, the muscle was examined histologically, using the gold chloride technique. Within 5 to 7 days

after the injection of saline homogenates of either normal or deneurotized rat sciatic nerve or limb muscle, about 0.5 per cent of the end plates in the test muscle developed ultraterminal branches. These ended blindly a short distance along the muscle fiber or in the surrounding endomysium. Occasional unconnected collaterals were also detected arising from the nerve fibers close to their end plates. Homogenates of rabbit spinal cord or brain produced similar results, but liver or heart homogenates were without effect.

Aqueous, alcoholic, and ether extracts of ox spinal cord were similarly tested. Of these, the ether extracts were most potent, producing ultraterminal branches on some 30 per cent of the end plates within 3 or 4 days of the injection. Ether extracts of normal or denervated rabbit muscle also proved to be active, as did extracts of egg-yolk. In the latter case, "up to 50 p.c. of the end-plates and many of the nerve-fibres produced long, blindly-ending outgrowths which twined around other nerve-fibres, approached end-plates, sometimes turning away, sometimes even fusing with the arborizations of other end-plates" (Hoffman, 1950, p. 394). Since tests with extracts of ox brain indicated that only white matter contains the active principle, Hoffman suggested that the latter is normally localized in the myelin sheath, from which it can be released either during Wallerian degeneration or by extraction procedures. To this hypothetical stimulating agent, Hoffman applied the term "neurocletin."

Further attempts at biochemical characterization have led Hoffman and Springell (1952) to infer that neurocletin is a moderately unsaturated fatty acid, perhaps related to the 18-carbon, cis-vaccenic acid whose hemolytic and respiration-inhibiting properties have been described by Laser (1949, 1951). Hoffman and Springell suggest that upon release of this fatty acid in esterified form from disintegrating myelin, it diffuses to adjacent intact axons, disrupts or disorganizes the axolemma and thus brings about the first stage of sprouting. Although a more complete appraisal of the specificity and chemical nature of neurocletin seems desirable, there is at present no evidence to support van Harreveld's (1952) suggestion that the occurrence of neurocletin in normal tissues may indicate an unspecific stimulating effect common to many compounds.

Sheath cells of Schwann. In his studies of residual medullated fibers in partially denervated tadpole tails, Speidel (1935, 1941) noted that the mitotic division of a Schwann cell was occasionally followed by a local retraction of the myelin sheath, the emergence of a collateral branch, and the development of a new node of Ranvier. In view of the rapid increase in the number of Schwann cells which occurs along the distal portions of mammalian nerve fibers during the first 3 to 4 weeks after nerve section (Abercrombie and Johnson, 1946; Thomas, 1948; Joseph, 1950), Edds (1950a) suggested that the stimulus for the collateral branching of intact axons might be associated with Schwann cell mitosis.

Subsequent investigations based on this premise have not produced any evidence in its favor. Hoffman (1952) has found that nitrogen mustards have no effect on collateral regeneration, although they inhibit the division of Schwann cells. A study of the effect of irradiation with x-rays on terminally regnerating rat nerves has also minimized the significance of Schwann cell mitoses (Edds, unpub.). When the tibial nerve is crushed following x-irradiation (2500 r) of the leg, Schwann cell proliferation distal to the crush is retarded. During a period of 3 weeks after crushing, the population of nuclei of all types in tibial nerves normally increases over 300 per cent; in irradiated nerves, the increase during the same time interval ranges from 30 to 50 per cent. The retardation of proliferation persists at least through the sixth week, if not longer. Presumably, this reduces the supply of cells available for transformation into macrophages (Weiss, 1949); but regardless of the cause, Wallerian degeneration is delayed and the neurilemmal tubes remain clogged with myelin and axonal debris for excessively long periods. As a result, the advance of regenerating axons is temporarily prevented. During the second postoperative month, after the intramuscular, neurilemmal tubes are finally cleared of myelin fragments, a few axons succeed in reconnecting with muscle fibers and begin to mature. When they have attained a diameter of about 4 μ, they begin to regenerate collaterally. The sprouts encounter and follow adjacent aneuritic sheaths, finally reentering denervated end plates. As other, previously blocked axons enter the muscle, some of them find the distal portion of their course preempted by collaterals. The latecomers nevertheless continue to advance and doubly neurotize end plates already innervated by collaterals (cf. Hoffman, 1951a, 1953a).

Three important conclusions emerge from these results: neither normal sheath cell divisions nor disintegrating myelin seem to be essential for the occurrence of collateral regeneration; whatever the source of the stimulus for sprouting, nerve fibers must attain a certain degree of maturity before they can respond to it; recently reinnervated muscle fibers are apparently less refractory to hyperneurotization than are those with fully differentiated end plates (cf. Weiss and Hoag, 1946; Aitken, 1950; and Hoffman, 1951a, 1953a).

Whether some other phase of Schwann cell activity is implicated in the causation of sprouting is still an open question. As Stroud and Brues (1951) have shown in tissue cultures of embryonic chick muscle cells, radiation-induced damage to the mitotic mechanism may prevent the actual separation of daughter cells without abolishing the locomotor activities which usually accompany cell division (cf. Stroud, 1952). Thus, even the mechanical stimulation which dividing cells might provide is not ruled out altogether (cf. Levi, 1926). Moreover, as the cytochemical studies of Hydén and Rexed (1943) and Holmgren and Rexed (1947) have shown, marked changes occur in the nucleoprotein metabolism and the metachromasia of hypertrophying Schwann cells during Wallerian degeneration. It is tempting to suggest that some phase of this altered metabolism leads to the elaboration of a neurocletin which, in partially denervated muscles, is released in the neighborhood of residual nerve fibers (cf. Tello, 1911).

Hoffman (1951b) has also admitted that a neurocletin-like substance may be produced by sheath cells. His evidence comes from a study of nerves implanted into normal muscles. He finds that nerves so implanted form transmissive connections with the already innervated muscle fibers. The new endings are small, atypical in form, and much less numerous than those formed by implants into denervated muscles (cf. Fort, 1940; Aitken, 1950). When, however, a normal muscle with a nerve implant lies next to a denervated muscle, the foreign axons form almost as many new end plates as they would if the test muscle were itself denervated (cf. van Harreveld, 1947). Hoffman infers that a readily diffusible humoral agent, derived from degenerating nerve fibers in the adjacent muscle, is responsible for this effect. That the agent is a product of nerve rather than muscle tissue is indicated by the observation that "uninnervated" muscle (i.e., a nerve-free portion of a muscle separated by a crushed zone from the region where nerve terminals occur) does not increase the number of connections established by nerves implanted into normally innervated areas. Since the stimulative action, whenever demonstrable, persists over relatively long periods, Hoffman suggests that the active principle may be released first from disintegrating myelin and subsequently from Schwann cells.

The nature of the action of this hypothetical substance on the outgrowth and connection of implanted nerves is obscure. It is unlikely that it increases the vigor and rate of outgrowth, as Hoffman (1951b) first contended. More recent experiments (Hoffman, 1953b) indicate that piromen (a bacterial pyrogen which inhibits cicatrization in transected spinal cords and permits enhanced regenerative activity; Windle et al., 1950, 1952) facilitates the invasion of normal muscle by implanted nerves, which then extensively hyperneurotize the muscle fibers. The agent from denervated muscle may have a similar action. Weiss (1950a) has given a comparable interpretation to the apparent enhancement of regeneration rate of severed corneal nerves reported by von Muralt (1946) following the administration of nerve extracts (neuro-regeneratory substance). However, there is no reason to deny categorically the existence of substances which exert specific growth-stimulating effects on nerve cells. On the contrary, mouse sarcomata elaborate agents which strikingly accelerate the growth and differentiation of sympathetic and some sensory neurons in the embryonic chick (Bueker, 1948, 1952; Levi-Montalcini, 1951; Levi-Montalcini and Hamburger, 1951). Even when the tumor is implanted on the yolk sac and has no direct contact with the embryo, sympathetic cells differentiate in abnormally large numbers, become hypertrophic, and invade organs which are not usually innervated (Levi-Montalcini, 1952).

The relatedness of neurocletin, the agent released from denervated muscle, neuroregeneratory substance, and the mouse sarcoma principle is a problem for future investigation. Their described properties and actions are sufficiently different, however, to make any close similarity improbable. Hoffman (1950, 1951b) has tentatively assumed the identity of neurocletin and the denervated muscle principle. This is a critical point. If these agents are identical, then neurocletin is readily diffusible, and the differential distribution of col-

laterals in a paretic muscle can hardly be attributed to local variations in the concentration of a sprout-evoking substance. If they are not the same, and if neurocletin diffuses sufficiently slowly or is destroyed rapidly, concentration gradients could be maintained with their highest levels in regions of greatest denervation. The number of sprouts arising from any parent fiber would then be a function of the number of degenerating fibers in the immediate vicinity.

This issue cannot be decided without further evidence. Moreover, as mentioned previously, there are two other mechanisms which might account for the differential distribution of collaterals. One of these would require a pattern of varying responsiveness to approximately equal amounts of neurocletin.

The response

Evidence bearing on the sprouting process viewed as a response of the intact axon to some external stimulus is available from experiments in which collateral regeneration was either retarded or accelerated.

Retardation of sprouting. Two mucopolysaccharides, heparin and sodium alginate, produce a marked inhibition of sprouting in paretic rat muscles during the first postoperative week (Hoffman, 1952). These experiments were prompted by the contention of Rexed and Rexed (1951) that no collateral regeneration of sensory axons occurs in the rabbit cornea after partial keratotomy. These authors found no Schwann cell proliferation under these circumstances and suggested an antimitotic action for the mucopolysaccharides which are abundant in the corneal stroma (cf. Balazs and Holmgren, 1949). Although Zander and Weddell (1951) believe that collateral regeneration does occur in the cornea, the fact remains that heparin and sodium alginate do at least delay this process in paretic muscles.

Hoffman (1952) has also demonstrated that colchicine retards sprouting, although, in exceptional cases, the reverse effect is obtained. In view of the ineffectiveness of nitrogen mustards, which are antimitotic yet fail to interfere with collateral regeneration, Hoffman argues that colchicine acts directly on the sprouting process rather than indirectly through damage to the proliferating Schwann cells. Since colchicine is believed to prevent the viscosity changes normally associated with cell division (Beams and Evans, 1940; Wilbur, 1940; Gaulden and Carlson, 1951), it may prevent axon sprouting by inhibiting some essential sol-gel transformation during the emergence of the collateral (cf. Lewis, 1950). A similar explanation can be advanced for the effect of mucopolysaccharides. Although heparin has long been known to block cell division (Fischer, 1930), its ability to prevent protoplasmic gelation and hence inhibit certain cellular locomotor activities (Heilbrunn and Wilson, 1949; Heilbrunn, 1952) permits the assumption that it also acts directly on the axon.

The precise nature of the heparin effect is unknown. Chaet (1952) has shown that the inhibition of protoplasmic clotting is due either to a breakdown product of heparin or to an impurity in commerical preparations. The following hints regarding the action of heparin should therefore be viewed with caution. The inhibition by heparin of membrane formation and cleavage in the sea urchin egg can be counteracted by treating the eggs with adenosine triphosphate (ATP) (Runnström and Kriszat, 1950; Harding, 1951; Runnström, 1952). Because of the antagonistic effects of heparin and ATP on amoeboid movement (Goldacre and Lorch, 1950), on blood clotting (Fischer and Schmitz, 1933) and on the contraction of actomyosin threads (McIntyre and Braverman, 1947), Harding (1951) has suggested that heparin may act by disturbing phosphorus metabolism at the cell surface. However, Anderson and Wilbur (1951) have shown that heparin causes the release from isolated nuclei of a gel containing desoxyribonucleic acid (DNA) and protein. In liver homogenates, the nuclear gel thus released produces a marked, but temporary, increase in viscosity. Anderson and Wilbur interpret their results as being due to a combination of heparin with basic proteins of the nucleus, DNA being replaced as a consequence (cf. Paff et al., 1952).

In view of these suggestions regarding the action of heparin, it is of interest that Gaulden and Carlson (1951) conclude that the lowered viscosity of colchicine-treated, grasshopper neuroblasts may be due to their reduced content of ribonucleic acid phosphorus. Szakàcs (1951) is also of the opinion that colchicine disturbs oxidative phosphorylations in developing toad eggs (however, cf. Hughes, 1952b, p. 260) while Bárány and Palis (1952) have shown that colchicine reduces the rate at which viscosity drops in mixtures of actomyosin and ATP.

In summary, if the collateral sprout arises in response to local disruption of the axolemma, then the effects of agents like heparin and colchicine presumably indicate that gelation phenomena are critical during the elongation of the sprout subsequent to the local solation which permits its initial break-through.

Enhancement of sprouting. A marked increase occurs in the rate of formation of collaterals in paretic rat muscles following electrical stimulation of either the lumbar spinal cord or the lumbar plexus (Hoffman, 1952). The time of onset of sprouting is not affected, but 4 days after partial denervation, muscles of the stimulated animals contain over 2.5 times as many new sprouts as do muscles of the untreated controls. Some 24 per cent of the deneurotized neurilemmal sheaths have been reinnervated in the former, as compared to about 9 per cent in the latter. The effect persists for several days, during which time the maturation of collaterals is also accelerated. Daily electrical stimulation of the paretic muscle itself apparently has no such effect (Kosman, Wood, and Osborne, 1948). These authors have shown that electrical stimulation retards the atrophy of denervated muscle fibers but, during the first 28 postoperative days, does not affect the tension developed by the whole muscle following indirect activation through its nerve. Presumably, therefore, the rapidity of collateral regeneration is not altered by the treatment.

Hoffman argues that the increased rate of sprout development and maturation is a consequence of an enhanced synthesis and distal flow of axoplasm (cf. Weiss and Hiscoe, 1948; Samuels et al., 1951). A greater source of axoplasm for the growth and maintenance of the new collaterals is thus provided, permitting a more rapid response to neurocletin. The argument is based on the metabolic changes believed to occur in the perikarya of electrically stimulated neurons (Hydén, 1943; Sulkin and Kuntz, 1950; Barr and Bertram, 1951; cf. Liu et al. 1950), as well as the fact that residual neurons first reveal the effects of stimulation during the fourth postoperative day, at a time when, according to Barr and Bertram (1951), recovery from stimulation-induced chromatolysis is beginning.

Hoffman's hypothesis makes the implicit assumption that the emergence of collaterals places a substantial drain on the axoplasm of the sprouting fiber. Actually, the new branches constitute only a small fraction of the entire cell. The volume of the latter is so great that axoplasm to fill even large numbers of collaterals would represent at most a few per cent of the total; in cells with long axons showing marked natural branching, calculated values for the share of collaterals in neuron volume amount to less than 1 per cent. Thus, if the axoplasm diverted into sprouts were drawn equally from all parts of the cell, only insignificant volume changes would occur either in the perikaryon or the axon. However, the axoplasm entering the sprouts cannot be drawn from the entire cell; nor can local deficits in the parent fiber, resulting from the siphoning off of its peripheral axoplasm, be expected to produce an immediate reduction in turgor pressure at more proximal levels, as would be the case in a liquid system. The column of axoplasm is not a liquid system, but has some degree of form stability, as Weiss and Hiscoe (1948) have emphasized. The results described by these authors imply that an elevated rate of cytoplasmic synthesis in the perikaryon would not increase the peripheral supply for many days, since the new axoplasm moves distally as a front advancing a few millimeters per day.

In this connection, it should be recalled that in cases where the size of nerve fibers has been related to the functional activity of their cell bodies, relatively long-term effects are involved. Thus, Fernand and Young (1951) have suggested that the caliber of a fiber may depend in part on the amount of afferent proprioceptive stimulation its cell body receives during development. Adult neurons which are functionally overloaded for 37 to 100 days, develop hypertrophic nuclei (Wendt, 1951) and axons (Wedeles, 1949; Edds, 1950b). Similarly, nerve cells which have supported new branches for several months have enlarged perikarya with normal patterns of Nissl substance (Cavanaugh, 1951). The axons of collaterally regenerating cells become hypertrophic after about 1 month, but they may even atrophy slightly during the first 2 postoperative weeks (Edds, 1949c; cf. Young, 1948). Therefore, it seems unlikely that electrical stimulation enhances sprouting during the first week through its effects on the synthetic activities of the perikaryon.

There is no reason, however, to deny that stimulation may accelerate the peripheral movement of axoplasm by speeding up the conveyor mechanism. If Weiss and Hiscoe (1948) are correct in their suggestion that this mechanism is a peristaltic wave along or in the fiber, stimulation might act either

by increasing its amplitude or rate. Since the conveyor mechanism is believed to reside in the entire axon, enlarged amounts of axoplasm would be made immediately available at the periphery. Possible changes in the cell body would then be irrelevant except for the long-term reequilibration of axoplasmic production and degradation. The demonstration by Grande and Richter (1950) that stimulation accelerates the peripheral displacement of P^{32} in frog nerve fibers can be adduced in support of this proposal.

One other possible explanation of Hoffman's results must also be considered, in view of the physical changes which occur in axons during the propagation of impulses. The literature on this subject has been critically appraised by Tobias (1952) and a detailed discussion would be out of place here. As Tobias points out, however, most of the available evidence indicates that axoplasm undergoes sol-gel changes during the transmission of impulses. He outlines an electrokinetic concept to account for these viscosity changes, as well as for the cyclical alterations which probably occur in, among others, osmotic and hydrostatic pressure, circulation of charged particles, and surface permeability to large molecules (see also Monné, 1949). If Tobias' argument is valid, it is reasonable to assume that the responsiveness of a nerve fiber to a sprout-evoking substance varies with its impulse-propagating activity. That is, electrical stimulation might produce a lowered threshold to the surface-disrupting action of some humoral agent. In this view, full responsiveness would simply develop more rapidly in stimulated fibers.

However, as Hoffman has shown, electrically stimulated neurons do not form collaterals earlier than normal. Increased sprouting activity first becomes evident on the fourth postoperative day, at a time when collaterals first appear in untreated, paretic muscle. Furthermore, daily stimulation during the presprouting period provokes no further increase in the number of new branches formed. Thus, if stimulation is effective by enhancing the responsiveness of residual fibers to some active substance, it must be inferred either that there is a prolonged delay between the initial action of that agent and sprout emergence, or that the fibers retain their increased susceptibility for at least 3 days.

Another treatment which accelerates collateral regeneration is the administration of pyronin. Hoffman (1952) reports that when this xanthene dye is given in drinking water (0.1% solution) to rats with paretic muscles, the sprouting of intact axons is greatly enhanced. Four days after operation, an average of over 40 per cent of the deneurotized sheaths are invaded by collaterals in the pyronin-treated cases. After 7 days, some 80 per cent of the available sheaths are reneurotized. Both rhodamine, a closely related dye (for formulae, see Conn, 1946), and methylene blue have no effect.

It is tempting to speculate that this finding implicates nucleic acids in the sprouting process. As is well known, pyronin is a valuable cytochemical reagent because of its affinity for ribose nucleic acids (e.g., Brachet, 1940, 1950; Michaelis, 1947; Pollister and Ris, 1947). The effect of pyronin on regeneration would be more understandable if it could be demonstrated that it influences the synthesis of nucleic acids in the perikaryon and ultimately the amount and rate of axoplasmic movement along the axon. However, the arguments already advanced seem to rule out this explanation for an acceleration of sprouting during the first week, and the mode of action of pyronin is therefore an open question.

Differential responsiveness. Nothing in the foregoing analysis explains the restrictedness of collateral regeneration. The results described merely provide clues as to the nature of the sprouting response; they do not reveal why some residual fibers produce more sprouts than others (unless fibers in more heavily denervated regions conduct more impulses per unit time, a point on which there is no direct information; but cf. Hodes, 1949; Hodes, Peacock, and Bodian, 1949; Denny-Brown, 1950; Edds, 1950b).

A differential distribution of collaterals would result if the parent axons varied in their sensitivity to roughly equal concentrations of neurocletin. But the problem of accounting for the origin of such a differential responsiveness is no easier than that of explaining the origin of a patterned stimulus mechanism. If a fiber is to sprout adaptively in accordance with local requirements, it must depend on information received from its immediate environment.

The difficulty can be illustrated in the following manner. Evidence is increasing that neurons have a high order of individual specification which enables them to form selective associations with other neurons (cf. reviews by Weiss, 1950b; and Sperry, 1951). If these differences permit not only

selective affinities and disaffinities but also interactions between adjacent fibers which thereupon become subtly altered, then the following argument would be reasonable. Since the fibers in terminal intramuscular nerve bundles represent the natural branches of several neurons (e.g., Wohlfart, 1935, 1949; Feindel, Hinshaw, and Weddell, 1952), the terminal branches of any one neuron are surrounded by varying numbers of branches of other neurons. Individual terminal branches therefore have different local environments. If by interacting with its neighbors, a fiber became conditioned in proportion to its numerical share in the total population of a nerve bundle, it might then acquire the ability to produce an appropriate number of sprouts if all its neighbors were destroyed. But if only some of the latter were destroyed, the fiber would then have to exhibit only a fraction of its sprouting potential. Determination of the size of this fraction would again depend on some outside signal. This would return the direction of sprouting pattern to some stimulus-mechanism external to the responding fiber where, as we have already argued, it can exist only if concentration gradients of a stimulating agent can be maintained.

Competitive sprouting

The difficulties inherent in schemes based either on a patterned stimulus or on differential responsiveness would be avoided if sprouting were a competitive process. The following proposal provides an example.

Conceivably, the release of neurocletin in the vicinity of intact axons causes all of them to begin sprouting. A given fiber under the influence of neurocletin has only a small probability of forming a collateral immediately, although this probability increases with time. The earliest sprouts reach empty sheath cell cords and upon contact begin to brake the stimulus-producing mechanism. As progressively larger numbers of collaterals emerge and neurotize adjacent cords, neurocletin slowly decreases in concentration until, at some critical level, sprouting ceases altogether. Meanwhile, rudimentary, still-unconnected sprouts are unstable and subject to withdrawal unless they encounter an empty sheath cell pathway; only successful competitors survive. Barring errors, the final regenerated system would inevitably compensate fully for the original loss of innervation.

Whether regenerating, but unconnected, axons are maintained or eventually withdrawn has often been debated. There is not even agreement on whether or not a recently and abnormally connected fiber will be withdrawn if its adopted terminal is invaded by the original fiber (e.g., cf. Zander and Weddell, 1951; Hoffman, 1953a). Unconnected, ultraterminal branches may persist for several days or weeks (Hoffman, 1950; Edds, unpub.), but unattached collaterals are not usually maintained, so far as the evidence goes (cf. Speidel, 1942). A reinvestigation of the early stages of collateral regeneration, considered from the point of view developed here, should be revealing.

The concept that competition and a feed-back control are involved in collateral regeneration is admittedly derived mostly from circumstantial evidence; it has been advanced mainly because explanations based on patterned stimuli or differential responsiveness appear inadequate. But the hypothesis does account for most of the known facts. It is consistent with prevailing views on terminal nerve regeneration, and it is amenable to further testing. Even if the proposed scheme proves to be correct, however, several problems will remain unsolved, for example: why do only the intramuscular portions of nerve fibers develop sprouts? how do the new branches effect penetration of the myelin and neurilemmal sheaths? what influence do they exert on reneurotized Schwann cells to inhibit neurocletin production? Only further experimentation can decide how relevant these questions are and how readily they can be answered.

CONCLUSION AND SUMMARY

The observations and experimental results which have been reviewed amply confirm Exner's (1885) suggestion that nerve fibers have a dynamic association with their periphery which enables them to exhibit adaptive, compensatory responses following the destruction of neighboring axons. Although these responses have been more fully studied in motor neurons (especially of the rat), they are also characteristic of sensory neurons, and perhaps of autonomic neurons as well (Hollinshead, 1948). They occur in all mammals so far studied, but are less easily demonstrable in the motor axons of primates. A few days after the surgical elimination of some of the fibers in a mixed nerve, and the concomitant partial denervation of a muscle, the residual intramuscular axons develop subterminal collateral or ultraterminal branches which enter the denervated regions and

reneurotize them (cf. Fig. 1). The amount of collateral regeneration is a direct function of the degree of original denervation; as a result, the affected muscle usually recovers most of its lost weight. Despite the enlarged motor units which compose it (and disregarding the probable, but poorly known alterations in its refined physiological properties), the muscle also regains the greater part of its contractile power.

Recent analyses of the causal factors responsible for the emergence of collateral sprouts from the residual axons, now permit us to go beyond the assertion that nerve fibers have sufficient turgor "to extend their terminals when not restrained from doing so by the presence of neighbouring nerve fibres" (Weddell and Zander, 1951, p. 247). The available evidence indicates that the sprouting phenomenon is essentially a response of the residual fiber to the solating action of a humoral agent released by adjacent, degenerating nerve fibers, probably from their transforming Schwann sheath cells. Localized disorganization of the axolemma is followed by the outgrowth of delicate axoplasmic strands which penetrate the nerve fiber sheaths (in a manner still unknown), establish contact with adjacent cords of Schwann cells (derived from degenerating fibers), and are thus guided to denervated end plates. Whether the guidance is primarily a matter of contact affinity or whether chemotropistic effects must also be admitted is undecided (cf. Edds, 1950; Hoffman, 1953a; Weiss, 1950a).

Although only the most distal (i.e., intramuscular) portions of intact motor fibers appear to be sensitive to the surface-disrupting agent, there is no indication that axons in one area of a muscle are or become more responsive to it than those in any other area. The biological and chemical properties of the agent indicate that it is a fatty acid which, perhaps in combined form, can diffuse extensively through the muscle and thus influence nerve fibers lying at a considerable distance. To explain how appropriate numbers of new branches are nevertheless formed in various local regions with differing degrees of partial denervation, a concept of competitive sprouting is proposed. In this view, which is appealing for its simplicity but which raises more questions than it answers, intact axons develop rudimentary collaterals in excess of local requirements. The new branches continue to elongate and reach size levels which make them readily observable in standard preparations only if they encounter an empty cord of sheath cells and succeed in connecting with a denervated end plate; failing this, they are mostly resorbed. Presumably, reneurotization halts the production by the sheath cells of the sprout-evoking agent and thus brings the sprouting process to a close.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will occasionally appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to H. B. Glass, Associate Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *Department of Biology, The Johns Hopkins University, Baltimore 18, Maryland, U. S. A.*

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GENERAL BIOLOGY: PHILOSOPHY AND EDUCATION

BIOLOGY AND LANGUAGE. *An Introduction to the Methodology of the Biological Sciences including Medicine. The Tarner Lectures, 1949–50.*

By J. H. Woodger. Cambridge University Press, New York. $8.00. xv + 364 pp.; ill. 1952.

This book is Woodger's first major publication since *The Axiomatic Method in Biology* (1937) and *The Technique of Theory Construction* (1939). It is designed, according to its author, to entertain and instruct readers who are interested in biological statements as well as in organisms. In it, as in his other methodological research, biological statements and theories are critically scrutinized, and their logical geography is mapped out with the help of special symbolic techniques supplementary to the technical resources of everyday biological language.

In Part I, biological statements are classified into two kinds: observation-records, obtainable by direct inspection of the objects constituting some domain of inquiry; and theoretical statements, or hypotheses. Theoretical statements that are generalizations of observation-records are said to be zero-level hypotheses; theoretical statements explanatory of these are said to be first-level hypotheses; statements explaining these in turn are said to be second-level hypotheses; and so on. A theory is regarded as a system of hypotheses each of which (save those belonging to the highest level) is logically deducible from one or more of the others.

The claim is made that all theoretical statements in biology can be formulated with seemingly meager linguistic equipment: (1) the operation of denying a statement by prefixing it with the idiom 'it is not the case that,' and the operations of combining two statements to form a third by writing between them one of the four idioms 'and,' 'or,' 'only if,' 'if and only if'; (2) the operation of universally generalizing, e.g., of prefixing the idiom 'no matter what x is chosen' to the expression 'x is red' to yield the (false) statement 'no matter what x is chosen, x is red'; (3) the identity sign '='; (4) parentheses; (5) variables replaceable by designative expressions that designate exactly one object; (6) numerals; (7) variables replaceable by numerals designating natural or rational numbers; (8) expressions peculiar to biology, or borrowed from the other sciences, that become designative expressions or statements when their variables are replaced by suitable designative expressions, e.g., as 'the male parent of x' becomes the designative expression 'the male parent of Darwin' when the designative expression 'Darwin'

replaces the variable 'x,' and as 'y is father of z' becomes, upon replacement of 'y' and 'z' by suitable designative expressions, the statement 'Erasmus Darwin is father of Charles Darwin'—such expressions are called 'functors'; (9) abbreviators, i.e., expressions introduced simply as shorthand for others already available; (10) variables that are replaceable by functors.

What this whole claim amounts to is that biological theory may be rewritten with the help of the idioms of mathematical logic together with some expressions belonging to the natural sciences; in the present volume it is defended chiefly by an extensive and detailed methodological analysis of genetics in Part II.

Genetic statements of zero-level are formulated with the help of (1) a mating-functor which is explained as designating the set of all offspring developing in environments of sort Z and that have a parent of sort W_1 that has developed in an environment of sort X, and a parent of sort W_2 that has developed in an environment of sort Y, and (2) the term 'phenotype.' With these two notations, statements informative of the genetical results of various sorts of matings (mating-descriptions) are formulated in complete generality; and such notions as those of dominance and recessiveness, homozygous genotype, heterozygous genotype, having the same linkage group, and others, are explicitly defined. For the first time in history, a precise interpretation of such familiar genetic formulae as "¼ A + ¾ Aa + ¼ a' is given, and a whole new branch of mathematics called "combined algebra" is created to deal with them.

First-level statements explanatory of mating-descriptions are formulated with the help of four new notations; (3) a functor designating the set of zygotes formed by the unions of gametes of sort α with those of sort β; (4) a functor designating the first temporal moment of a time-extended object; (5) a functor designating the set of gametes produced by organisms of genotype Y when they develop in environments of sort X; and (6) a functor of the form of 'x is the environment of y.' With these functors, and some others defined by means of them, statements are constructed that speak of gametes and their union to form zygotes developing (relative to specific environments) into lives belonging to some phenotype. Some of Mendel's laws appear among these statements. Special hypotheses are stated that connect these first-level functors with those on zero-level and from which zero-level mating-descriptions are shown to be logically deducible (provided that certain statements are adopted that specify antecedent conditions under which genetic theory is to hold). Woodger claims to have formulated on this level a complete generalization of Mendelism—but this claim perhaps involves an element of convention, for thus far no mention of Mendel's "hereditary factors" has been needed.

The analysis begins to enter cytogenetics proper with four new notations: (7) a functor of the form of 'x is part of y'; (8) a functor of the form of 'x is a nucleus and y is a cytoplasm and each is part of the same living cell'; (9) a functor of the form of 'the cell y is one of two daughter cells into which the cell x has divided'; and (10) a sign designating the set of cells in which first reduction-divisions occur. These notations prove surprisingly fertile in defining expressions designating various sorts of nuclear and cytoplasmic sets relevant to genetic theory, but they are not shown until later to be explanatorily powerful, although special postulates are adduced linking them with functors of lower levels.

Some additional functors are now introduced on level three: e.g., (11) an undefined functor of the form of '$x_1, x_2, \cdots, x_n$ are n chromosomes all parts of one and the same nucleus which has only n chromosomes.' But again, although a number of definitions are given that connect this functor with ones on lower levels, we are not shown until later how they, the postulates that govern their use, and the statements in which they occur together yield mating-descriptions as logical consequences.

Instead, there is introduced (12) a fourth-level functor of the form of '$x_1, x_2, \cdots, x_m$ stand in the correct relation to constitute a chromosome,' and the author turns to the problem of distinguishing on this level between genetic systems with linkage groups and those without. Postulates are introduced to regulate the use of (12), and definitions are constructed (e.g., of expressions designating the set of genes correlated with a given phenotype and of expressions designating chromosomal loci) that link it up with functors on lower levels. The author shows how these expressions would differ in connection with genetic systems of the two types mentioned above; and then proceeds to show what hypotheses are required in order that certain first-level hypotheses may be derived—and here at last we are shown how second- and third-level functors are employed in the derivations.

We are next treated to an account of expressions that the author regards as having been responsible for controversy and misunderstanding in genetics: 'heredity,' 'hereditary character,' 'acquired character,' 'inheritance,' 'determines' or 'is due to,' and 'is identical with.'

This completes, essentially, Woodger's treatment of genetics. It should be remembered that only twelve ***undefined*** biological functors are employed for all this —but not ***undefinable***, for the author shows in notes supplementing this section how many of these are explicitly definable.

In Part III, we are offered some suggestions for the methodological treatment of neurology and medical psychology: among other items of interest there is an intriguing set-theoretical analysis of the reflex concept, a down-to-earth discussion of problems involved in the study of perception, and a moving commentary on the

central hypotheses of Suttie's *Origins of Love and Hate.*

There are 3 long appendices: the first is designed to exhibit the logical structure of William Harvey's blood-circulation theory; the second expounds an extremely interesting methodological tool (which the author calls "the theory of the joining of sets by one-one relations") for the study of evolutionary theory; and the third is entitled Natural Science and Human Affairs.

It is to be hoped that biologists will have the imagination and industry to benefit from Woodger's enormous creative efforts on behalf of their language; for if it is desirable to replace diagrams, ellipses, and metaphors with explicit statements, and if it is desirable to clear away the verbal trash defacing our theories, and if it is desirable to exhibit the minimum commitments of our science and to expose its theoretical structure clearly—then it is certainly desirable to pay some serious attention to the best attempts that have yet been made to attain these methodological ends.

JOHN R. GREGG

PROBLEMS OF LIFE. *An Evaluation of Modern Biological Thought.*

By Ludwig von Bertalanffy. John Wiley & Sons, New York. $4.00. xi + 216 pp. 1952.

According to the author, the most important antithesis of modern biological thought is that between mechanism and vitalism. These antithetical views are to be reconciled by the application of still a third *Weltanschauung*, for whose existence the author takes full credit, and which is called by him "the organismic conception." The organismic conception is to appear fully-fledged, in a forthcoming volume by the author, as a mathematically formulated general theory of systems. It is especially designed to do justice to the teleological behavior of living systems. At the same time, it purports to avoid the alleged disaffection of vitalism from empirical restrictions and to remedy the explanatory poverty ascribed to mechanism. Only brief mention is made of current theoretical developments undertaken in complete independence of the organismic conception, but which are nevertheless already attaining the ends to which that view is dedicated: for instance, research in cybernetics and in the thermodynamics of open systems. And still other important investigations in this area are not mentioned at all.

JOHN R. GREGG

DOUBT AND UNCERTAINTY IN SCIENCE. *A Biologist's Reflections on the Brain. The B. B. C. Reith Lectures, 1950.*

By J. Z. Young. Oxford University Press, London. $2.50. viii + 168 pp. 1951.

Psychology is one of the youngest of the sciences. Up to the last turn of the century it was generally considered a branch of philosophy rather than of science, and its development was retarded accordingly. One of the first attempts to place psychology on a scientific basis was made by the behaviorists, but the followers of this school of thought tied their own hands by ruling out introspection as an instrument for the acquisition of knowledge, and so after a brave beginning lost themselves in aimless wandering along a blind by-path. Since then the tendency has been for each individual psychologist to conduct his investigations independently, making no attempt to integrate his work with that of his contemporaries. Much that is useful and valuable has been learned, but it has been only within the last few years that a serious attempt has been made to integrate these findings into a discipline. The author of the work here under discussion is a pioneer in this field.

The series of eight Reith lectures of which this work consists are an attempt to answer the question, How does the brain work? Until we know a great deal more about the brain it is futile to discuss the mind. The brain is a physical organ and can be studied in the same way as the other physical organs can be studied, but it has never received the attention necessary to serve as a foundation for a scientific psychology. The author disclaims being a physiologist, yet he has done a remarkable piece of work in assembling and unifying the work of past and present scholars whose investigations have thrown light on the way in which the brain functions. It is of course not possible in a series of lectures to expound all these preliminary data—the exigencies of time preclude such a course. But the author has sought to remedy this defect by expanding the book with supplementary chapters of comments on the lectures. It would perhaps have been better had the author rewritten the work entirely, explaining in detail the contributions made to his thought by the 25 or more authorities to whom he acknowledges his indebtedness in the Preface. It is to be hoped that in the near future the author will attempt such a task, even if it involves expanding the book to ten times its present size.

SYMMETRY.

By Hermann Weyl. Princeton University Press, Princeton, New Jersey. $3.75. vi + 168 pp. + 1 pl.; text ill. 1952.

Weyl's book has a double appeal. It is beautifully illustrated with examples of symmetry chosen from art and science, and it discusses these examples in terms of the abstract mathematical structure common to them

all. It is therefore strongly reminiscent of D'Arcy Thompson's *On Growth and Form* and will raise the same doubts in the mind of the biologist: is there any value in the attempt to refer complex phenomena to an abstract unifying theory when so little is known of their proximate causes? The author's answer seems to be that of Thompson: the fact that we find these symmetries thus related challenges us to seek explanations in the deeper symmetries of physical cause and convinces us that those explanations exist. The book provides sheer visual pleasure, many intriguing ideas, and insight into a mode of mathematical formalism (theory of finite groups) quite different from the statistics and differential equations familiar to biologists. About one-third of the book is mathematics, the remainder is equally divided between physical science, biological science, and art. Each of these subjects is discussed with fluent reference to the others. No admirer of Thompson's book should miss Weyl's.

PAUL CRANEFIELD

PHILOSOPHY OF MATHEMATICS AND NATURAL SCIENCE. *Revised and Augmented English Edition Based on a Translation by Olaf Helmer.*

By Hermann Weyl. Princeton University Press, Princeton; Geoffrey Cumberlege, Oxford University Press, London. $5.00. x + 311 pp.; ill. 1949.

It has taken the reviewer the better part of a year to read this book. This comment is not intended as any denial of the very real accomplishment represented by this book. The author has undertaken the difficult problem of dissecting "reality" in an incisive way. Nor is my comment intended to discourage prospective readers. Any really searching examination of the basis of scientific knowledge is bound to be both disturbing and discouraging. No simple or even obvious general principle emerges—on the contrary, the examination underscores, in a disturbing way, the importance of subjective factors such as intuition in any inquiry into epistemology.

This book appeared originally in a German edition in 1926, and its author has added several appendices dealing with the structure of mathematics, quantum physics and causality, chemical valence and the hierarchy of structures, physics and biology, and morphe and evolution. It is in these appendices that the reader will find a formulation of some of the most interesting problems confronting science today.

L. J. MULLINS

MAN ON HIS NATURE. *Second Edition. The Gifford Lectures, Edinburgh 1937–8. By Sir Charles Sherrington. Cambridge University Press; London, New York.* $6.00. vi + 300 pp. + 8 pl. 1951.

In the 11 years that have passed since the appearance of the first edition of this elegant work, Sherrington's views on biology and on man have apparently undergone some change. The book is about 100 pages shorter than its first edition, and one feels that both from the standpoint of what has been omitted, and from text revisions, the author has embraced a more mechanistic view of biology. The Preface states merely that the new edition has given the opportunity for a certain amount of revision as regards detail, but it appears that more than detail has been changed. The descriptive passages are as beautiful as before, and any change in viewpoint expressed is a subtle rather than an obvious change.

L. J. MULLINS

HUME: THEORY OF KNOWLEDGE *Containing the Enquiry concerning Human Understanding, the Abstract, and selected passages from Book I of A Treatise of Human Nature.*

Edited by D. C. Yalden-Thomson. Thomas Nelson & Sons, London, Edinburgh, Melbourne, Capetown, Toronto, and New York. 7s. 6d. xxviii + 265 pp. 1951.

HUME: THEORY OF POLITICS *Containing A Treatise of Human Nature, Book III, Parts I and II and thirteen of the Essays, Moral, Political and Literary.*

Edited by Frederick Watkins. Thomas Nelson & Sons, London, Edinburgh, Melbourne, Capetown, Toronto, and New York. 7s. 6d. xxx + 246 pp. 1951.

These two pocket-sized volumes provide the best of David Hume's thought, skilfully edited and with excellent introductions by the editors. It is the first of the two volumes that will have most appeal to scientists. Here one finds most clearly stated Hume's theory of knowledge, which has had so potent an influence upon the development of the philosophy of science. The attention of those who have not read them is particularly directed to the chapters Of Probability, Of the Idea of Necessary Connexion, Of Liberty and Necessity, Of the Reason of Animals, and Of Miracles, in the *Enquiry*; and those entitled Why a Cause is always Necessary, and Of Scepticism with Regard to the Senses, in the *Treatise*. Not a few biologists, in reading these pages for the first time, will be surprised to find what a debt their own ideas of the nature of things and the web of cause and effect owe to that master of skepticism and suspended judgment, the 18th century Scotchman, David Hume.

BENTLEY GLASS

BERKELEY—PHILOSOPHICAL WRITINGS.

Selected and edited by T. E. Jessop. Thomas Nelson & Sons, Edinburgh. 10s. 6d. xxv + 278 pp. 1952.

For those who do not desire the full set of Berkeley's works so superbly edited by A. A. Luce and T. E. Jessop, and already reviewed here (*Q. R. B.*, 25: 206. (1950); 26: 367. (1951); 27: 201. (1952); 28: 160. (1953)), one of the two editors has prepared this selection, which is adequate to convey the main thought of the philosopher. The two major works, the *Treatise Concerning the Principles of Human Knowledge*, and the *Three Dialogues between Hylas and Philonous*, are almost complete. The *Essay towards a New Theory of Vision*, which would most interest biologists, is more abridged, but the main argument is intact. The pocket-sized volume also contains parts of *De Motu*, the second dialogue from *Alciphron*, and part of *Siris*. An Introduction by the Editor, a Chronological Table of Berkeley's Life and Writings, and a Bibliographical Note complete the volume.

BIOLOGY, THE STORY OF LIFE.

By Ernest S. Booth. Pacific Press Publishing, Mountain View, Calif. $4.50. x + 710 pp.; ill. 1950.

Science in general, and biology in particular, can but suffer when a book such as this is published and used for a textbook in secondary schools. It was prepared under the direction of the Seventh-Day Adventists, who accept the literal Biblical account of Creation, and do not believe that man has evolved from a lower form of life. According to the Foreword, one of the objectives of the book is to equip the student with the scientific method for interpreting the world in which he lives. But the concept of the scientific method that the student of this textbook must acquire would be a strange one, if indeed he has any at all.

A number of the chapters in the book are quite commendable—good format, excellent illustrations, and an interesting style of writing. But where the subject matter of biology seems to be in conflict with the Bible, then the Bible must be right, in spite of the facts. As might be expected, the interpretation of variation in nature and the history of life on the earth receives rather thorough "Creationist" treatment. In these chapters, the book has little resemblance to a biology textbook at all, for here the evidence is primarily from the Bible; or when these quotations do not suffice, from a large array of Seventh-Day Adventist writings, all of which apparently have equivalent authority. The Flood is described in great detail, because to Booth the fossil-bearing strata are nothing more than layers of sediments produced during the Flood (shades of Cuvier), with the entire geological record the result of a passage of time of *one year and ten days*!

This book is perhaps even more objectionable than those common a generation ago, because Booth "recognizes" the progress of modern science, and with a potpourri of religious quotations, biological facts and half-truths, and geological absurdities, tries to compress it into a narrow fundamentalist interpretation. In recognizing the reality of organic change, he states, "Thousands of . . . different kinds of plants and animals in the world today have come into being *since* Creation and *since* the Flood." But where man is concerned, fossil men indicate tremendous degeneration from the "image of God."

The ends to which Booth has gone to explain the world of life are often deplorable. On bird migration: "Evidently God placed certain instincts in some of our birds which cause them to migrate regularly from north to south, and back again. How else can we explain it?" On endocrine glands: "We may also marvel at the wisdom of our Creator who made them so that after six thousand years of degeneration and disease they still work properly most of the time." On cell division: "Mitosis and gametogenesis are two of the strongest arguments for Creation."

In a chapter on healthful living, Booth advises against the eating of meat, since meats are diseased, contain poisons, and lead to cancer: "It has been found that races of people who eat meat a good deal have cancer much more often than do those races of people who do not eat meat. While we do not yet have *experimental* proof that eating of meat produces cancer, the Spirit of Prophecy plainly states that meat actually does have that effect." It is impossible to justify such a statement under any conditions.

The student who wants to do further reading on any topic in this book will find good lists of references at the ends of many chapters, but nothing except "creationist" references following those chapters dealing with the history of life on the earth.

In order to justify his "philosophy of science" on bases which are entirely authoritarian, the author shades, distorts, misrepresents, or completely ignores tremendous bodies of scientific facts. Such action by a supposed scientist, in order to compress a truly magnificent "story of life" into the impossibly narrow limits of a religious doctrine, justifiably warrants his inclusion with those Soviet scientists who have done the same thing in order to comply with a political doctrine. Their exclusion from the body of self-respecting scientists must inevitably follow.

FRANK C. ERK

BIOLOGY: HISTORY AND BIOGRAPHY

BIOLOGIE DER GOETHEZEIT.

Edited by Adolf Meyer-Abich. Chronica Botanica

Co., Waltham, Massachusetts. $5.50. 302 pp.; ill. 1949.

Biology in the early 19th century was dominated in Germany by the spirit of the Naturphilosophie. It is difficult to comprehend that bygone outlook on biological problems without going back to the original representatives and comparing and contrasting their views. Hence this work, which reprints in the German 7 essays or groups of selections from Goethe, Forster, von Humboldt, Oken, Carus, von Baer, and Müller, is a prize indeed for everyone interested in the history and development of biological concepts.

The work includes a preliminary Introduction, on the subject Goethe's Natural Research in its History-of-Ideas Form, and a Conclusion dealing with Goethe's Principle of Compensation, the First Holistic Basic Principle of Modern Biology, by the editor. Between these one finds: Selected Writings on the Philosophy of Morphology and on General Morphology (Goethe)—14 selections occupying almost exactly half of the book; A Glance into the Entirety of Nature (Georg Forster); The Life-Force, or the Rhodian Genius (Alexander von Humboldt); Development of a Scientific Systematics of Animals (Lorenz Oken); What are the Requirements for the Future Development of the Natural Sciences? (Carl Gustav Carus); What is the Right Conception of Living Nature? (Karl Ernst von Baer); and Regarding the Requirement on the Part of Physiology for a Philosophical Conception of Nature (Johannes Müller). Each selection is accompanied by a biographical essay by the editor and by a plate of the subject. There is unfortunately no index, a lack which seriously impairs the value of the volume as a reference work. Otherwise, it leaves little to be desired.

BENTLEY GLASS

THOMAS JEFFERSON: SCIENTIST.

By Edwin T. Martin. Henry Schuman, New York. $4.00. x + 289 pp. + 15 pl.; text ill. 1952.

From a consideration of the origin of his interest, it might be argued that the pursuit of science served Thomas Jefferson for two purposes. For the practical Jefferson, science facilitated the improvement of the American economy. He devoted much time in America and Europe to investigations of new crops or machines that might be put to profitable use by the young nation. Such was his zeal in these matters that we see him, then Minister to France, easing out of Italy with pockets filled with contraband seed, an act of smuggling for which the penalty could have been death. His proclivity toward practical science has brought us many benefits, among them the decimal coinage system and the Weather Bureau.

To the political Jefferson, science was often an escape, a relatively serene world into which he might retreat when political life became unbearable. While Martin amply documents Jefferson's desires to deflect political trouble by a retreat to scientific pursuits, the most striking instance has been omitted. In brief, this occurred during Jefferson's tortured service as Governor of Virginia when, with the British ready on the James River to disembark an army to which there could be little practical resistance, with his Council and Legislature scattered and unwilling to cooperate, Jefferson came close to washing his hands of the Revolution and retiring to Monticello, the more comfortably to indulge his scientific nature. Fortunately, he endured those desperate times, and when he undertook the formidable business of reducing the Comte de Buffon to size, even his scientific pursuits came to reflect a little of the urgency of the day. Their controversy, relative to Buffon's dogma that climate had rendered the New World fauna inferior to that of Europe, is perhaps the most interesting aspect of Martin's fascinating study, *Thomas Jefferson: Scientist.*

J. F. CASE

NUTTALL'S TRAVELS INTO THE OLD NORTHWEST. *An Unpublished 1810 Diary. Chronica Botanica, Vol. 14, No. 1/2.*

By Jeannette E. Graustein; edited by Frans Verdoorn. The Chronica Botanica Company, Waltham; Stechert-Hafner Company, New York. $3.00 (paper). viii + 88 pp. + 9 pl.; text ill. 1950/51.

When Thomas Nuttall died in 1860 every newspaper in Philadelphia, where for so many years he had made his home, published an obituary notice. Like so much that we read in our own daily press, these were compiled by writers who did not appreciate the importance of accuracy, with the result that biographical accounts of his career, drawn largely from these same notices, are strangely inconsistent. Two such were reviewed in these columns (*Q.R.B.*, 18: 309. 1943) and the discrepancies between them pointed out.

To a large extent Nuttall himself was responsible for our lack of precise data about his life. He was extremely careless, especially in the matter of dates, according to the editor of this diary, now published for the first time. It is one of the only two of his manuscripts known to be in existence today. Further, Nuttall did not have a good literary style. His diary is deadly reading, consisting almost exclusively of names of places visited and plants seen on the way, and dates of arrival and departure. To cap the climax, the diary breaks off in the middle of a sentence, in Missouri, just when his travels were beginning to be interesting.

Thus it will be seen that the title *Nuttall's Travels into the Old Northwest*, by which the editor designates this narrative, is a misnomer. Nuttall did get into the

Northwest eventually, it is true. He reached the site of Helena, Montana, only five years after Lewis and Clark, and was captured there by hostile Indians who threatened him with death. He purchased his freedom by taking his watch apart and presenting a wheel to each of his captors. But by this time he had long since ceased to keep a diary.

What would we not give today for a first-hand account from his pen of the villages of the Mandan Indians in South Dakota? He seems to have been the best qualified of the few scientifically trained men to visit this interesting tribe before it was wiped out by an epidemic of smallpox. According to the more or less unreliable testimony of a few travelers, these Indians differed widely in cultural development from those of Plains Indians by whom they were surrounded. The Mandans dwelt in frame houses with roofs of grass thatch instead of in tepees covered with hides, and they practiced agriculture instead of depending on the hunt. Their folklore seems to indicate that their ancestors had come across Hudson's Bay, a fact which has led to much idle and fruitless speculation as to their origin. Nuttall might have obtained information that would have helped solve the mystery, but he fumbled.

The editor apparently shares the reader's disappointment with this diary, for he has supplemented it with a readable account of Nuttall's career and several pertinent photographs, among them one of Nut Grove, Lancashire, where Nuttall spent his last years. I had the privilege of visiting this estate last summer. It is now deserted, the school for girls which was its last occupant having evacuated it some years ago. The grounds are neglected, but some of the trees which Nuttall knew and which he may have planted, are still growing. The shade of Thomas Nuttall himself is not there. Somewhere on a remote coral island in the tropical seas it is tramping the beaches, gathering new species where no man has ever been before him.

JOSHUA L. BAILY, JR.

THE LIFE AND WORK OF ASTLEY COOPER.

By R. C. Brock. The Williams & Wilkins Company, Baltimore. $4.50. viii + 176 pp. + 18 pl. 1952.

All those who are conversant with the development of biological science during the first half of the 19th century recognize the great stature of Astley Cooper. His definitive biography by Bransby Cooper, his nephew, most eloquently testifies to this. But, since its publication in 1843, the fame of Astley Cooper has been overshadowed by that of his great predecessor and contemporary, John Hunter. This misfortune has prompted the present reintroduction to readers of a great scientist and surgeon.

The Preface expresses the fear that the account may be too laudatory, and praise of Astley Cooper is indeed the chief characteristic of the book. One wishes that a more serious attempt had been made to evaluate Cooper's work in relation to the march of progress in his main field, surgery. Some chapters are devoted to his work on arterial ligation for aneurysm and amputation, his chief contributions. But the main interest is found in colorful stories illustrating his versatility and remarkable boldness of attack, no matter what the problem. There was his dubious employment of "resurrection men" which led to the passage of the celebrated Anatomy Act. On another occasion an elephant was dissected in the open courtyard of his home because it was too big to get into his dissecting room. No less fascinating is the part Cooper played, posthumously, in the strange case of Napoleon's (?) intestines. Despite a code of ethics that might be called rough and tumble by modern standards (to say the least), Cooper's full and active life bears the hall-marks of greatness. Originality, courage, boldness, and skill were all required of a surgeon in the grim days before general anesthesia, and Astley Cooper was the greatest surgeon of his time.

Brock's biography is personal rather than scholarly, being written by a faithful son of Guy's Hospital and its medical school in praise of a great predecessor on its staff. But it should find a comfortable place in the library of those interested in the development of surgery and of biological science in general.

F. N. LOW

EDWIN CHADWICK AND THE PUBLIC HEALTH MOVEMENT, 1832–1854.

By R. A. Lewis. Longmans, Green & Company, London, New York, and Toronto. $5.00. viii + 411 pp.; 1952.

The public health movement in Great Britain, which was born about 100 years ago amid the squalor and filth of London, stands today as a monument to the ideals, the initiative, and the ingenuity of one man—Edwin Chadwick. Working in an age when time-honored custom was the modus vivendi, and laissez faire was the daily watchword, Chadwick not only showed that social and sanitary reforms were necessary, but designed some practical schemes for their accomplishment.

Chadwick realized that the first step toward controlling social evils was to measure them, and although his level of understanding of vital statistics never reached that of his contemporary, William Farr, he made good use of figures in laying the foundation for his public health reforms. With every one of his ideas for the improvement of health had to come a plan for administration, because the legislators of his day moved with considerable complacency in the deep grooves of time-honored habit and comfortable think-

ing. Chadwick's health movement came into continuous conflict with the Malthusian theory which held that because of a trend toward over-population, the high death rate, which was admittedly associated with poor sanitary conditions, was not only good, but a necessary factor in society.

The *Sanitary Report* demonstrated the need for improvement in water supply, sewerage disposal and better housing conditions. The *Health of Towns Commission Report* proposed ways and means of accomplishing the needed improvements. The third phase of Chadwick's work was concerned with translating recommendations into legislation. This was accomplished in 1848 by the passage of the First Public Health Act, which provided for a Central Board of Health in the Government, and gave the cities and towns authority to set up local Boards responsible for local water supply, sewerage disposal, paving, street cleaning, etc.

In the accomplishment of his work, Chadwick identified himself with two unpopular propositions, 1) that local representative bodies were not always competent to administer, without the aid and supervision of the Central Board, all health matters which affected their districts; and 2) that profit-making bodies were not invariably the best instruments for rendering service to the public. Against these doctrines ranged the many advocates of local self-government and private enterprise. Although the storm of opposition eventually resulted in the fall of the General Board of Health, Chadwick lived long enough to see his position completely vindicated in the permanent establishment of a Governmental Department of Health.

This volume will undoubtedly have a popular reception among public health workers all over the world. An extensive bibliography is appended.

B. AUBREY SCHNEIDER

THE YOUNG NATURALIST

MAC MALLARD.

By R. W. Eschmeyer; designed and illustrated by Francis W. Davis. Fisherman Press, Oxford, Ohio. 50 cents (paper); $1.00 (cloth). iv + 50 pp.; ill. 1953.

WILLIE WHITETAIL.

By R. W. Eschmeyer; designed and illustrated by Roy K. Wills. Fisherman Press, Oxford, Ohio. 50 cents (paper); $1.00 (cloth). iv + 49 pp.; ill. 1953.

AL ALLIGATOR.

By R. W. Eschmeyer; designed and illustrated by Roy K. Wills. Fisherman Press, Oxford, Ohio. 50 cents (paper); $1.00 (cloth). iv + 49 pp.; ill. 1953.

WOODY WOODCOCK.

By R. W. Eschmeyer; designed and illustrated by Francis W. Davis. Fisherman Press, Oxford, Ohio. 50 cents (paper); $1.00 (cloth). iv + 50 pp.; ill. 1953.

R. W. Eschmeyer has done an excellent job in presenting the four readable and accurate accounts of the lives of these animals. Written for children, the approach is neither sentimental nor patronizing; in fact, the directness of approach, coupled with excellent illustrations, is sufficiently well handled to make any additional appeal to the young reader superfluous. These small volumes make a good contribution to our conservation literature.

C. P. SWANSON

LIVING THINGS.

By Frederick L. Fitzpatrick and Thomas D. Bain. Henry Holt & Company, New York. $3.60. xiv + 415 pp.; ill. 1953.

This is a textbook for students in secondary schools. As the authors point out, such a book must meet the needs of students whose backgrounds, abilities, and aims are extremely varied. To meet these needs, a book must be scaled to the average student both in content and in language. It seems that the present volume meets the objectives set forth. It is therefore an adequate book, but like the students it seeks to reach, it is also a very average one.

C. P. SWANSON

ECOLOGY AND NATURAL HISTORY

ZOOGEOGRAPHY OF THE SEA.

By Sven Ekman. Sidgwick & Jackson, London; [The Macmillan Company, New York.] $6.50. xiv + 417 pp.; ill. 1953.

The hope that Professor Ekman's *Tiergeographie des Meeres* would appear in English has at last been realized, to the great satisfaction of all students of marine zoogeography. More than realized, for instead of resting on his well-deserved laurels, the author has rewritten the entire volume in Swedish and had it translated from his manuscript into English, thus avoiding the stiffness which undoubtedly would have blighted a translation made directly from his German edition. The language is simple and easy—a credit to the translator—and the publishers, Sidgwick and Jackson, have added another indispensable volume, the 10th, to their well known series: Textbooks of Animal Biology.

The format of the book is smaller than that of the original German edition and the number of illustrations have been sharply reduced to keep down the bulk of the volume—as well as the price.

Although the pattern of the first edition is followed, the author has made many changes, making extensive use of the progress of our knowledge in this field during the last 17 years, progress which I am sure has in many cases been inspired by his earlier great work. Notable is his treatment of the crustaceans, based in several instances on the collections of the Hancock Expeditions and the work of the younger American workers. Also important is his inclusion of, partly unpublished, work, dealing with the Gulf of Mexico—that almost unknown region of the western hemisphere. It is quite evident that he is still "in the midst of the battle," and that he has the good will of many marine biologists.

In such a work, which encompasses the zoology of all the oceans, there is of course an enormous amount of detail which needs to be worked over, in spite of the almost unbelievable amount of first-hand knowledge the author has had in a large number of fields. Some specialists will undoubtedly find statements which they feel should be strongly modified, or completely withdrawn as incorrect. At the same time, they will here and there find their own theories backed up by the evidence presented in other groups. The person who most of all realizes the flaws in the work is undoubtedly the author himself, and I am fairly sure that he is the one who most eagerly will welcome corrections, as well as additional information about the various animals. In spite of this last edition being written by a professor *emeritus*, there runs a youthful eagerness through the pages which gives the book a vigor one rarely finds in a textbook. I have no doubt that it will soon be out of print, and I am sure that the author would welcome the opportunity to bring a third edition up to date, and that it would be just as enjoyable as the present volume.

ELISABETH DEICHMANN

This book is a remarkable achievement. Few of us, in this specialized age, can realize the amount of work involved in such a synthesis as this, and the author deserves our admiration for carrying on his work beyond the usual requirements of a new edition. Many parts have been extensively rewritten, and criticisms of the earlier edition have been accepted as guides for revision rather than bones of contention. Minor errors are inevitable, and omissions of work in particular groups will occur to specialists, but serious mistakes are few. One such is the characterization of *Ostrea virginica* as "amphi-American" (p. 33); another, the use of biocoenosis for commensalism (p. 6), may be a translator's error. The section on brackish water fauna is restricted to the Baltic and northern Europe, and the distinction between brackish and estuarine faunas is not drawn, a failure which limits the value of the discussion of brackish water and estuarine distributions. In my opinion, the statistical method of determining faunal provinces proposed by Schenck and Keen is discussed somewhat uncritically, while the zonal scheme of Hutchins is ignored altogether. Nevertheless, the book is crammed with useful generalizations and concepts and is an essential reference for the shelf of every student of the seas and its life.

Recognizing the confusion in terms which has arisen in recent years, Ekman has taken a stand in this edition which should be called to the attention of ecologists and paleontologists. The term littoral, which in the 1935 edition included the entire continental shelf, is here restricted to the intertidal region, and "shelf" is used for the "fauna of the sea-floor from the inter tidal zone as far as the outer edge of the shelf." Neritis is reserved for the water above the shelf, as a "planktological" term. In fact, terminology has been kept to a minimum in this book, and its approach is best described in the author's own words: "marine zoogeography must primarily be the zoogeography of the various waters, and only secondarily a zoogeography of the various coastal regions."

JOEL W. HEDGPETH

LES CONDITIONS ECOLOGIQUES ET DE PEUPLEMENT DES VASES D'EAU DOUCE. *Encyclopédie Biogéographique et Ecologique, VI.*

By Franklin Pierre; Preface by Jacques Bourcart. Paul Lechevalier, Paris. 1,800 fr. (paper). 109 pp. + 8 pl.; text ill. 1951.

A thesis on the composition and physical characteristics of freshwater muds (in France) and their colonization by organic life. Classifications of muds by particle size and chemical composition and ecological succession in some selected environments are discussed.

GRUNDRISS ZU EINER BALNEOBIOLOGIE DER THERMEN. *Lehrbücher und Monographien aus dem Gebiete der Exakten Wissenschaften.*

By Vale Vouk. Verlag Birkhäuser, Basel, Switzerland. Sw. Fr. 11.50 (cloth); 8.05 (paper). 88 pp.; ill. 1950.

Information concerning peculiar environments such as thermal waters is as widely scattered as the springs themselves. Hence it is very useful to have such information brought together in such a compact form as this monograph. Since the author is a professor of botany, special attention is given to the algae of hot springs, and the animals are less thoroughly treated. A number of schemes of classification of hot springs is offered, according to temperature and to algal types. As the purpose of this book is to synthesize the biological peculiarities of hot and mineral springs with a view to understanding their therapeutic properties, it seems

strange to find so little about the chemical characteristics of thermal springs. A few tables of analyses would have added greatly to the usefulness of this monograph.

JOEL W. HEDGPETH

THE IMMACULATE FOREST. *An account of an Expedition to unexplored territories between the Andes and the Amazon.*

By Dr. W. R. Philipson. Philosophical Library, New York. $4.50. 223 pp. + 15 pl. 1952.

From his personal account of a dry-season expedition into the virgin jungles of the Macarena Mountains just east of the Andes in southern Colombia, it is clear that Philipson is just the man we needed with us in Panamá. Biologists who have clambered, as we did, over the trails in the Canal Zone Biological Area—becoming increasingly aware that the life of the jungle was above them in the remote interlacing tree tops—will relive their experiences with new delight through reading these glowing pages and learning the names of the trees they walked among. In the tropics so much of the vegetation reaches tree size, and the smaller plants grow as epiphytes on the larger, that man finds himself looking up and feeling a lack of animal companions on the jungle floor. The expedition led by Philipson had the advantage of exploring virgin territory and making their own trails in regions where not even local Indians were present to make the wildlife wary. How the animals responded to so much felling of trees to get flower specimens and hence to learn the identity of each trunk, is clear only in that the ornithologist of the group worked off in separate directions in order to obtain specimens. Certainly a wide variety of monkeys and birds were studied, and encounters with jaguars, bears, electric eels, stingrays, and herds of white-lipped peccaries enliven the tale. This is a fine book for those who contemplate a jungle adventure, and for those who have experienced life in a tree-clad tropic land.

LORUS J. & MARGERY J. MILNE

VIE ET MOEURS DES MAMMIFÈRES.

By F. Bourlière. Editions Payot, Paris. 900 fr. (paper). 250 pp.; ill. 1951.

A compendium of information on locomotion, digestion, territoriality, reproduction, development, migration, and special ecological aspects, viewed internationally, with selected bibliographies for each chapter and a longer one at a general level. The index is useful, and the illustrations are better than might be expected from the quality of the paper in this paper-bound volume.

LORUS J. & MARGERY J. MILNE

POSSUMS.

By Carl G. Hartman. University of Texas Press, Austin. $6.00. xiii + 174 pp.; ill. 1952.

If this volume had been entitled *The Opossum*, it might well have been a very different book—a factual and sober account of everything that has been learned in four and a half centuries about these strange animals and their marsupial relations. And such a book the author is well qualified to write. However, in calling it *Possums*, he clearly reveals his intentions and also something of himself, as one not given to using two words if one will serve, or a long word if there is a shorter one.

This is a book that covers the history, natural history, mythology and folk-lore of the opossum and his kin, with a consideration of his personal habits and behavior, and something of his—and her—physiology. There is also included the possum's picture gallery, the possum's garden of verse, and at least the beginnings of the possum's own cook-book. All this is set forth in an easy and sprightly style, so that perhaps the most serious criticism is that there might well have been more. The historical chapters are excellent. Drawing directly from contemporary sources and using freely of contemporary illustrations, they revive the records of the discovery, naming, and scientific appraisal of a new and amazing animal. At the same time they manage to convey a lively impression of the quality of biological science in the 17th and 18th centuries. Those were times when new beasts were nothing new, but none the less exciting. It appears that Ferdinand and Isabella saw and personally examined the first opossum brought from the New World, and even inserted royal fingers in the pouch, thus setting a precedent for biologists, and especially embryologists, ever since. It is a coincidence worth noting that the history of the discovery of marsupials in a sense recapitulates their evolutionary history, the New World forms monopolizing the scientific scene for more than a century before the advent of their more numerous and diversified relatives from "down under."

The book has much to say about the physiological and psychological peculiarities of the opossum—his unique solutions for the most ordinary problems of living—and much, too, of things that need to be unsaid. Animal behavior is peculiarly liable to human interpretation, and perhaps no type of misinformation is more difficult to dispel than traditional beliefs about behavior and its motivations. That truth is often stranger than fiction does not always make it more acceptable. A case in point is the notion, so fully recorded in pictures, that the mother opossum habitually transports her young, suspended from her tail by theirs. How this legend arose, was handed down, and wonderfully improved on from book to book, is well depicted. The universal belief that "playing possum" is a conscious and crafty act of deception is also dealt with,

and a more probable explanation is advanced. The fascinating story of how the new-born and seemingly helpless young make their unassisted journey to the pouch is told again; but apparently this story cannot be repeated too often, since after many years simple truth still fails to replace ancient fiction in many quarters where it ought to be well known (see, e.g., *Text-Book of Zoology*, Parker and Haswell, Vol. II, 6th ed., 1940, and many another textbook and encyclopedia).

In spite of an ingrown personality and an almost perfect incapacity for friendship the opossum appears in proper perspective, for the author understands him even when he cannot wholly admire him. But for all this our hero still has need of an attorney. Any unbiased examination of his private life and character based on personal acquaintance inevitably leads to derogatory conclusions. Admittedly his intellectual equipment is meager. His cranial capacity, compared with creatures of his size, might well be cause for embarrassment; but although this deficiency may account for the vacant quality of the famous grin, it probably has nothing to do with its sheepish quality. For in fact the opossum has been a great practical success, and there is little to indicate that weakness of intellect has been a serious handicap through the ages. A living fossil he may be, but he is not in a hurry, and everything is grist that comes to his mill. Face to face with a chicken coop or a grape arbor he knows automatically what to do. So here he is today, apparently flourishing and even extending his empire, having outlasted dinosaur and pterodactyl, mastodon and saber-tooth; and he bids fair to outlast some others.

The book is handsome in design and well printed, with a good index and a selected bibliography especially rich in historical titles. An attractive feature is the unusual number of fine pictures, a text of 162 pages being illustrated with more than 100 photographs and reproductions. In this gallery opossums are not the only delectable characters. To any southerner of a certain vintage and given to occasional nostalgia, the picture on p. 148 alone justifies the book. Here is a page of the living material that inspired the tales of Uncle Remus and the unforgettable drawings of Frost and Kemble.

R. K. BURNS

HAMMOND'S NATURE ATLAS OF AMERICA.

By E. L. Jordan, with the assistance of a group of specialists in the various branches of the natural sciences; 320 original paintings, 282 by Walter Ferguson and 38 by John Cody. C. J. Hammond & Company, Maplewood, N. Y. $7.50. 256 pp.; ill. 1952.

There is a wealth of nature volumes available to the general reading public, both amateur and professional. Competition for sale is therefore intense, and the trend has been toward bigger and more profusely illustrated texts, although not necessarily more erudite or more useful. The present volume is an example of this effort to gain the reader's attention. Actually it adds very little to the library of the nature lover that cannot be gained from much less expensive books. When one considers that the value of an atlas is measured by its usefulness, this one falls down. Except for the maps which survey the facilities for nature observation and enjoyment, and which do serve a useful function, the remainder of the volume is taken up with a rather garish and not too well-done display, at the rate of two pictures per 9 x 12 inch page, of selected examples of minerals and rocks, trees and flowering plants, mammals, birds, fishes, insects, amphibians, and reptiles. Distribution maps go with each group, but the number of species discussed must obviously be reduced to only the commonest forms, since only a little over 300 total species are mentioned. A child would probably get a lot of enjoyment, and possibly some information, from the idle turning of the pages, but an adult hardly. The volume ends with a Classification Key which purports to "show the place which each individual species occupies in the whole of our natural environment." As a means of identification the key is useless, since it consists only of a listing of various rock forms or of plant and animal families. Consequently it permits one only to read more about those forms with which one is already familiar. Such a book is likely to contribute but little to the further education of those who use it.

C. P. SWANSON

EVOLUTION

ORIGIN AND EVOLUTION OF MAN. *Cold Spring Harbor Symposia on Quantitative Biology, Vol. XV.*

The Biological Laboratory, Cold Spring Harbor, L. I., New York. $7.00. xii + 425 pp. + 3 pl; text ill. 1950.

The Cold Spring Harbor symposium of 1950 attests to the extent to which the point of view and approach of genetics have become a part of physical anthropology. Since the latter is by its very nature an eclectic science it was inevitable that it would eventually apply genetical methods to the solution of some of its problems. It was also inevitable that geneticists would extend their sphere of research beyond such animals as Paramecium, Drosophila, and the rabbit and hence intermesh their interests with those of the anthropologists. This symposium bears eloquent testimony to the extent to which this has occurred. Here the anthropologist and the geneticist met on common ground and discussed common problems in a common language.

The many intriguing and difficult problems of human origin and evolution, both phylogenetic and racial, are particularly well adapted to a genetic approach. Indeed, this symposium makes it evident that if these problems—or at least many of them—are ever to be solved it is by such an attack. Moreover, one of the most encouraging things to emerge from this meeting is the evidence that the "type" concept—that perennial plague of physical anthropology—is now of little more than historical interest. Instead, physical anthropologists are thinking in terms of "population." This applies not only to living human races or other groups but also to other living primates and, quite as significantly, to fossils as well.

The 28 papers and 9 discussions from the chair that comprise the symposium were arranged in 9 sessions, as follows:

"Population as a Unit of Study": Genetics and the origin and evolution of man (H. H. Strandskov); Genetic structure of natural populations and interbreeding units in the human species (A. Buzzati-Traverso); Problems and methods of population surveys (F. P. Thieme); Concluding remarks of the chairman (M. Bates).

"Origin of the Human Stock": The specializations of man and his place among the catarrhine primates (A. H. Schultz); Some principles of historical biology bearing on human origins (G. G. Simpson); The analysis of primate evolution with particular reference to the origin of man (S. K. Washburn); Concluding remarks of the chairman (W. W. Howells).

"Classification of Fossil Men": The genus Palaeoanthropus and the problem of superspecific differentiation among the Hominidae (T. D. McCown); The problem of the earliest claimed representatives of Homo sapiens (T. D. Stewart); Taxonomic categories in fossil hominids (E. Mayr); Concluding remarks of the chairman (W. M. Krogman).

"Genetic Analysis of Racial Traits (I)": Clinical and genetical entities in human populations (J. A. Böök); The frequency of diseases affected by heredity in Denmark (T. Kemp); The population genetics of two inherited blood dyscrasias in man (J. V. Neel); Concluding remarks of the chairman (L. H. Snyder).

"Genetic Analysis of Racial Traits (II)": Blood groups, morphology and population size of the Eskimos (W. S. Laughlin); Genetics of three normal morphological variations: Patterns of superficial veins of the anterior thorax, peroneus tertius muscle and number of vallate papillae (J. N. Spuhler); Genetic analysis of racial traits of the teeth (G. W. Lasker): Concluding remarks of the chairman (S. C. Reed).

"Genetic Analysis of Racial Traits (III)": The eight blood group systems and their inheritance (R. R. Race); The blood groups of the peoples of the Mediterranean area (A. E. Mourant); Three general types of racial characteristics (W. C. Boyd); Concluding remarks of the chairman (C. P. Oliver).

"Race Concept and Human Races": Human races in relation to environment and culture with special reference to the influence of culture upon genetic changes in human populations (C. S. Coon); Some implications of the genetical concept of race in terms of spatial analysis (J. B. Birdsell); A consideration of the concept of race (M. F. A. Montagu); Anthropological maps of the Nordic countries (B. Lundman); Population size and microevolution in Greece (J. L. Angel); Concluding remarks of the chairman (L. C. Dunn).

"Constitution": The constitutional modification of Mendelian traits in man (S. M. Garn); Constitutional aspects of juvenile delinquency (C. C. Seltzer); The somatotype, the morphophenotype and the morphogenotype (W. H. Sheldon); Concluding remarks of the chairman (E. A. Hooton).

"Perspectives of Future Research": Human diversity and adaptation (T. Dobzhansky); Population genetics and social anthropology (C. Kluckhohn and C. Griffith); Concluding remarks of the chairman (C. Stern).

Space does not permit consideration of all of these papers, but some that particularly interested me may be noted.

Thieme's Puerto Rican survey contains a timely warning of the factors that make difficult the securing of valid population samples—geography, climate, parasites, nutrition, cultural forms, etc.

Washburn's stimulating paper presents a strong argument for a genetic and functional analysis of primate evolution, using experimental methods whereever possible. I am quite sympathetic with Washburn's general point of view, even though I do not agree with some of his conclusions respecting the course of human evolution. Howells clearly outlines the major problems to be solved before the origin and early evolution of the human stock is actually pieced together; after reading this discussion I cannot help but reflect upon how little is actually known and how much remains to be known. On the other hand, Stewart's critical and illuminating discussion of the known fossil remains of actual man indicates that here factual knowledge, although admittedly quite incomplete, outdoes plausible theory. Mayr's redefinition of the genus *Homo* represents, in my opinion, an unwarranted instance of extreme taxonomic "lumping" that is quite as unrealistic as the extreme "splitting" to which he and other systematists have objected. His rather astonishing suggestion that man and the African anthropoids be united in a single family (to the exclusion of the other living anthropoids) recalls Weinert's facile interpretation of the evidence in creating a similar taxonomic group, the "Summoprimates," and is another example of the all too frequent mental acrobatics to which a superficial knowledge of Primates can lead. An erect, bipedal posture, with its accompanying new adaptive zone, can more meaningfully be used as the prime criterion of the family Hominidae rather than, as

Mayr uses it, as the criterion of the genus *Homo*. The one peculiar distinction of the latter is a degree of cerebral development resulting in production of a culture, as I have noted elsewhere. Leaving aside the above points, another real difficulty of Mayr's suggested classification is that it forces one into a trinomiality (and, conceivably, even into a quadrinomiality) that is as objectionable as the extreme "splitting" against which he rightfully protests.

Spuhler's study of three normal morphological variations and Lasker's analysis of racial dental traits are good examples of what can be done by a combined knowledge of genetics and human anatomy. The papers of Laughlin, Race, Mourant, and Boyd emphasize the current importance of the blood groups in anthropology.

Coon's paper demonstrates the influence of culture upon the genetic constitution of human populations and is a timely warning that man cannot be dealt with as a biological organism alone. Cultural factors that influence the selection of mates, the ability of the female to reproduce, the ability of offspring to reach reproductive age, the reproductive longevity of the individual, and the ability of the individual to influence group survival, are all shown to be of prime importance in their effects upon racial composition. Birdsell's genetical analysis of aboriginal Australian populations is an outstanding contribution that emphasizes the concept of space as an important variable in the genetical analysis of race. He explains certain of the features under consideration on the basis of drift and other ones on the basis of selection. Using gene-flow models, he ingeniously presents a new hypothesis respecting the time and place of entry of the Carpentarian element into Australia.

The papers dealing with various aspects of constitution, by Garn, Seltzer, and Sheldon, indicate that this sort of approach holds promise for the future. But it is also clear that in methodology this field is still largely in its infancy. Nor is there yet, despite Sheldon's facile assurance, a satisfactory framework of theory. Certainly, the relative importance of heredity and of environment in producing the variations in constitutional types remains an unsolved problem. The validity of much of what has been said about somatotypes depends upon the results of its solution.

That only some of the papers have been discussed should not be taken as an implication that the others lack equal interest and merit. Indeed, the symposium as a whole is unusually high in quality. If it can be taken as an omen, the cross-fertilization of physical anthropology and genetics should yield a rich harvest.

WILLIAM L. STRAUS, JR.

EVOLUTION. *Second Edition.* *By A. Franklin Shull. McGraw-Hill Book Company, New York, Toronto, and London.* $5.00. x + 322 pp.; ill. 1951.

This is a revised edition of a textbook which first appeared in 1936 (*Q.R.B.*, 11: 341. 1936). It has the merit of clarity. It begins with a discussion of the evolution of the theory of evolution from Thales of Miletus to the present day. About half the book is devoted to a discussion of genetics, the advances in which during the past few years have made this revision necessary. The remaining half of the book is made up of a discussion of the contribution to evolutionary thought of such allied sciences as embryology, paleontology, comparative anatomy, sociology, and geography, and the final chapter is devoted to a discussion of the origin of life. This last is the weakest part of the book. About all the author can tell us is that until an environment capable of supporting life had been prepared, life could not appear.

Shull has brought up several controversial matters which are worthy of some consideration. "A family," he says, "arises by the breaking up of . . . the species of a genus into new species." Is it not rather that new families are caused by the divergence of genera in old families, each genus passing as a whole to one of the resultant families? Shull does not believe the elytra of beetles aid in flight. It is true that the elytra do not vibrate during flight, but neither do the wings of aeroplanes. At least one pioneer in the history of aviation has identified the mechanical principle of aeroplane flight with that of the Coleoptera, on the ground that the outstretched elytra perform the same function as the aeroplane wing, and that if deprived of them the beetle could not leave the ground.

Does the common occurrence of gastrulation in animals imply a common ancestry, as Shull implies? Most animals do pass through a gastrula stage in their embryonic development, but the process by which the blastula is converted into a gastrula differs widely in different groups. If phyla could be discriminated on the basis of differences in method of gastrulation, it would be quite justifiable to cite the gastrula as evidence of common ancestry, but the gastrulation process has almost no taxonomic significance whatsoever. The gastrulation of the cephalopod is identical with that of the fish and altogether different from that of the snail, yet the cephalopod is indubitably a mollusk despite the absence of spiral cleavage and trochosphere and veliger stages in its development. It is very difficult to regard the gastrulations of such organisms as *Loligo* and *Lymnaea* as modifications of the same fundamental process, and to attribute their differences to differences in quantity and distribution of yolk within the egg, as orthodox embryologists attempt to do.

Finally, it is also true that there are certain impressions in Cambrian rocks which resemble the parapodia of pteropods, and that have been so interpreted. But the recent pteropods originated in the Cretaceous, and if these are the progeny of so-called Paleozoic pteropods

one would expect to find a connecting series of forms extending across the intervening strata. Actually, the fossils which are relied upon to connect the Paleozoic and Cretaceous pteropods resemble the specialized forms in the Cretaceous rather than the generalized forms, and some of them resemble belemnoids more than pteropods. Further, they differ among themselves so much that it is doubtful if some of them are mollusks at all. Shull may be correct, but if the presence of pteropods in the Cambrian should ever be substantiated it would precipitate a deep-seated revolution in molluscan taxonomy and overthrow all the classical notions of molluscan evolution.

GESCHICHTE DER PELANZEN.

By Walter Zimmermann. Georg Thieme, Stuttgart. DM.10.-(paper). xii + 111 pp.; ill. 1949.

In this little book the author of the telome theory attempts to record the entire history of the plant kingdom by typifying the 10 major steps of phylogenetic significance. Not counting conceivable prototypes of life, the first phylogenetic level is represented by unicellular and filamentous algae and bacteria lacking nuclei and sexuality, and is followed by levels two to four, characteristic of flagellates and thallophytes. Levels 5 to 10 represent milestones in the phylogeny of vascular plants (Rhyniaceae to Angiospermae). Various diagrams and figures illustrate these types. General principles, such as the alternation of generations, are discussed in a concluding chapter. A glossary of terms with their definitions is appended. A convenient English summary can be found in Zimmermann's own review of the "Main Results of the 'Telome Theory' " (*The Palaeobotanist*, 1: 457–470. 1952).

THEODOR JUST

EMBRYOS AND ANCESTORS. *Revised Edition.*

By G. R. de Beer. Oxford University Press, London. $3.00. xii + 159 pp. + 2 pl.; text ill. 1951.

de Beer's slender book, now enlarged somewhat over the 1940 edition (*Q.R.B.*, 16: 100. 1940), is recognizably the best modern account of the status of Haeckel's recapitulation theory, the so-called biogenetic law. It is hence surprising that most modern writers on evolutionary theory, from Dobzhansky to Schmalhausen and Rensch, seem to have ignored or disregarded it, although taking a similar position. The genetic and evolutionary study of the various types of modification of the animal's developmental pattern from egg to adult should be a fertile field for experimental investigation, along lines here foreshadowed.

The book itself has changed little except for the addition of certain new chapters. These consist of papers written by the author during the past decade, published in various books or journals, and now included here without always fitting too well into the framework of the original treatment of the subject. There is a 2-page chapter on The Evolution of the Coelenterates which adds little, and two chapters of more substance, on Embryology and Taxonomy, and on Embryology and Homology, respectively. Neither is really up to date. The former could have benefitted from a study of Ernst Mayr's *Systematics and the Origin of Species*, even if de Beer were to disagree with the viewpoints developed there; and the second essay has ignored the enlightening discussion of homology during the past few years by Boyden, Haas and Simpson, Moment, Szarski, and others. The best of the book is still the original part.

BENTLEY GLASS

GENETICS AND CYTOLOGY

UNDERSTANDING HEREDITY. *An Introduction to Genetics.*

By Richard E. Goldschmidt. John Wiley & Sons, New York; Chapman & Hall, London. $3.75. x + 228 pp.; ill. 1952.

This book is an elementary introduction to the field of genetics. It addresses itself to the layman, including students of precollege age. It is different from a college textbook since it does not attempt to cover the whole subject matter of genetics, but is restricted to the classical chromosome theory of heredity. No attempt is made to present the overwhelming amount of material now accumulated in the field of genetics, but simple striking cases are selected for the demonstration of the phenomenon discussed. In this way the logical conciseness and beauty of the genetical theory comes out more clearly than in many more advanced works. The book may, for this reason, be very suitable as a high school textbook.

The material covered in this book includes elementary Mendelism, with two- and three-factor crosses, factor interaction, and multiple factor inheritance. Another series of chapters deals with the behavior of chromosomes, the chromosome theory of heredity, linkage and the mapping of chromosomes, and sex-linked inheritance. One chapter on mutations is included. A final chapter gives a cursory survey of some of the more advanced topics of genetics. It seems to be intended to whet the appetite of the reader to delve more deeply into the study of heredity. At the end of the book are added (1) a small number of problems, (2) a list of more advanced books on genetics, (3) a glossary, and (4) an index. The illustrations are mostly

schematic, except for one plate showing microphotographs of chromosomes and mitosis.

It is well known that the author of the book holds opinions concerning the nature of the gene which are at variance with those of many other geneticists. It should therefore be emphasized that no inkling of these disputed opinions of the author appears in this book. Both the facts included and their interpretation will not be challenged by any present-day geneticist. This is due to the fact that the author restricts himself to the discussion of the secured facts of fundamental genetics, and avoids those subjects on which agreement amongst the active workers in the field has not yet been reached.

This does not mean that the author does not mention any controversial subjects. He expressly states in the Preface that the reappearance of the hypothesis of the inheritance of acquired characters, in Russia, has induced him to publish the present book. This hypothesis is concisely and clearly disproved at the end of the first chapter. The author seems to feel, however, that the most convincing refutation of Lysenkoism consists in an understanding of the clarity and simplicity of the chromosome theory, and its ability to explain all known facts of heredity. I feel that this clear, easily understandable, and well-supported book on genetics will contribute significantly to the defense of that science against the Lysenkoist attack; and I hope, therefore, that the present book will find its way into the hands of a large number of non-geneticists.

The book is based, both with regard to the material covered and to the way of presentation, on an earlier book by the same author which appeared in German in the nineteen-twenties. Having read this book as a high-school student, I am therefore able to attest that the book can be easily understood and appreciated by children of high-school age without any previous knowledge of biology. I would also like to take this opportunity to acknowledge that the reading of this book was one of the major factors which induced me to choose genetics as my own particular field of study. I hope that its present successor in English may have a similar effect on present-day high-school students.

ERNST CASPARI

GESCHLECHT UND GESCHLECHTSBESTIMMUNG IM TIER- UND PFLANZENREICH. *Sammlung Göschen 1127.* *By Max Hartmann. Walter de Gruyter & Co., Berlin.* Dm.2.40 (paper). 116 pp.; ill. 1951.

This small pocket-size book on sex-determination is addressed to the educated layman and should be easily understandable by anybody interested in biological problems. The wealth of material included and the clear and well-supported presentation of the author's original views on the determination of sex make interesting and rewarding reading matter for the professional biologist.

The main body of the book falls into 4 chapters: genotypic and phenotypic sex-determination in the haplo- and in the diplophase, respectively. Two introductory chapters explain the technical terminology used and present evidence that bisexuality, i.e., the fusion of cells of opposite sexes, is a general biological phenomenon. A short chapter on the sex hormones in vertebrates is included. A final chapter presents the author's general theory of sexuality in coherent form, after the material on which it is based has been presented all through the preceding chapters. The book closes with a glossary and an index. A large part of the subject matter presented in this book is derived from the original research of the author and of his students notably Moewus. There is unfortunately no extensive bibliography which would make it easier for the professional reader to follow up the interesting facts presented in the book. There are many illustrations, most of them pen-and-ink drawings made after illustrations in the original papers.

The main point the author makes is the proposition, first suggested by Correns, that every organism possesses fundamentally a bisexual potency symbolized by the letters AG. The actual sex developed depends on sex-"realisators" which suppress the potency for one sex, leaving the other one active. These "realisators" may be genes or environmental factors. A great part of the discussion centers around the question whether it is necessary to assume the bisexual potency AG in addition to sex-determining factors, or whether it is not sufficient to take the genetic and environmental factors as determining the sex. A large number of cases is cited to support the first view. Amongst the haploid organisms, the author finds the situation of relative sexuality, as found in algae such as *Chlamydomonas*, *Ectocarpus*, *Protosiphon*, and in some fungi (*Glomerella*, *Sordaria*) particularly convincing. In some of these organisms, sex is determined by two linked genes, *F* for femaleness and *M* for maleness, but the sexual behavior of the organisms is dependent on the "strength" of the *M* or *F* alleles present. An organism carrying a weak *M* allele is male and acts as such against female gametes having the same or higher strength; but it behaves as a female in the presence of strong male gametes. In other words, the organism, though determined to be a male by carrying an *M* gene, has not completely lost its ability to react as a female. Amongst diploid organisms, the ability of genetically female *Melandrium* plants to form anthers when infected with *Ustilago* is interpreted to mean that in *Melandrium* females the potency to form male organs is maintained but suppressed under normal conditions. The author cites as particularly strong evidence for his point of view the transformation of corn from a monoecious to

a dioecious plant, accomplished by Jones by means of the two genes *ts* and *sk*, which individually affect only the differentiation of the male and female reproductive organs. It is to be regretted that the author does not discuss under his point of view the sex-determination in *Habrobracon*, which has been so thoroughly worked out by Whiting and his collaborators.

A question which arises from the author's view regarding sex-determination is whether in hermaphroditic organisms sex "realisators" for both sexes are present, or whether they contain only the bisexual potency AG without any "realisators." The author leaves this question open, but seems to incline towards the first alternative, since in *Chlamydomonas* the crossovers containing *F* and *M* in the same chromosome are hermaphroditic with phenotypic sex determination, while the complementary crossovers lacking both genes are lethal.

The book should prove interesting to every biologist, since it contains in clear, concise, and well-organized form a large amount of fundamentally important subject matter. It should furthermore prove highly stimulating and thought provoking since the reader will have to take a position to the author's interpretations and the evidence on which they are based.

ERNST CASPARI

FORTPFLANZUNG IM TIER- UND PFLANZENREICH. *Sammlung Göschen, Band 1138.*

By J. Hämmerling. Walter de Gruyter & Co., Berlin. DM 2.40 (paper). 135 pp.; ill. 1951.

The task of this book, to present in a limited amount of space a synopsis of the processes of reproduction found in organisms, appears formidable. It is obviously necessary to concentrate on certain aspects of the field, omitting others. The author has solved this problem admirably by restricting himself mainly to the presentation of the variety of reproductive processes found in animals, plants, and unicellular organisms from a morphological point of view. The problem of sex-determination is omitted, since it is discussed thoroughly in a companion volume by Hartmann, appearing simultaneously in the same collection. A few selected topics from the physiology of reproduction are discussed in a much shorter second part.

The main part of the book, dealing with the morphology of reproduction, is divided into 2 chapters, dealing with asexual and sexual reproduction, respectively. The first of these chapters includes mitosis, the differentiation of asexual spores in the Thallophyta, and the multitude of machanisms of asexual reproduction in higher animals and plants. The second chapter treats systematically the processes of gamete formation and fertilization, and proceeds to describe various forms of parthenogenesis, and the alternation of haploid and diploid generations. The physiological part treats 3 subjects: the physiology of cell division, the physiology of fertilization, and the physiology of alternation of generations. None of these subjects is treated exhaustively, as may be seen from the small amount of space allotted to them. The discussion of the physiology of fertilization is of particular interest, since it summarizes shortly and concisely the biochemical work done by Moewus on substances active in the copulation of *Chlamydomonas*, and by Hartmann on fertilizins in sea urchins.

The main value of the book lies in the systematic treatment of the various forms of reproduction found in different organisms. The amount of material is enormous, but due to the clear and concise description by the author, it is easy to grasp and to organize. The author does not try to present any general hypothesis, and mentions his own important contributions to the field only shortly and casually. The material mentions the more recent developments in the field, such as reproduction in bacteria and bacteriophages as well as the more classical studies, particularly in the Protozoa and Thallophyta. It does not, however, put any particular stress on the recent contributions, leaving the main emphasis on the older observations.

A study of the book leads to the striking impression, how many well-established facts from the older literature have been forgotten by present-day biologists. It is a pleasant and valuable experience to be reminded of the large amount of work one has not thought of for some time. The book should prove particularly valuable to people who want to acquaint themselves in a short time with a large amount of material. Particularly will graduate students enjoy the well-organized presentation of a large number of observations which can lead them on to the study both of the older and of the modern literature in the field.

ERNST CASPARI

DIE TECHNISCHEN GRUNDLAGEN DER KINEMATISCHEN ZELLFORSCHUNG. *Vorschläge für eine exakte wissenschaftliche Mikrokinematographie.*

By Willi Kuhl. Springer-Verlag, Berlin, Göttingen, and Heidelberg. DM.26.00 (paper). viii + 185 pp.; ill. 1949.

A useful addition to the technique of cinephotomicrography is the author's contribution of rotating sectors of various designs for minimizing heating effects on the biological material and at the same time allowing great flexibility in the control of lapse-time cinephotography. These sectors revolve in front of the light source and trigger the film feed of the cinecamera at appropriate intervals. This manual contains many practical hints

for photomicrography, and some idea of the types of experimental investigations that are of necessity studied with the rather elaborate instrumentation described.

L. J. MULLINS

FOREST TREE BREEDING. *Two Volumes.*

By Keiji Sato. Asakura Book Company, Tokyo. Y. 420 per vol. Vol. I: ii + vi + 238 pp.; ill. Vol. II: ii + ii + vi + 239–521 pp.; ill. 1950.

These two volumes on forest-tree breeding constitute Part V of the *Complete Works of Silviculture*. The first volume deals primarily with the theoretical problems of tree breeding. It comprises the following 7 chapters: Special Features of Tree Breeding; Reproduction; Variation; Statistical Studies of Variation; Heredity; Chromosomes; and Races of forest trees. A comprehensive systematic list of chromosome numbers of woody plants with authorities cited is appended in Chapter 6. The list includes a number of Asiatic woody plants not listed in Darlington and Janaki's *Chromosome Atlas*.

The second volume is devoted to three practical problems of tree breeding. (1) The distinction of forest-tree races. The various static methods (morphological, anatomical, cytological, physiological, etc.) and the experimental methods (plantation test, progeny test, and experimental breeding) are discussed. (2) The selection of forest-tree seeds. The age and site conditions of seed trees, effect of thinning and selective cutting, and the practical measures relating to the procurement and control of seed supply are examined in this chapter. (3) The last chapter deals with the methods of tree breeding. Discussions are included on selection, hybridization, artificial induction of mutation by radiation, and, to be thorough, a 3-page discourse on the virtues and techniques of creating so-called "graft-hybrids."

The author presents a comprehensive review of the literature on genetics and cytology in general, and on tree breeding in particular. One of the special values of this work, particularly to non-Japanese geneticists, is the summation of current Japanese literature which is not readily accessible.

The entire text is written in Japanese, but in most cases scientific names of plants are also given.

WANG CHI-WU and SCOTT S. PAULEY

GENERAL AND SYSTEMATIC BOTANY

LEHRBUCH DER ALLGEMEINEN BOTANIK. *Band I. Morphologie, Anatomie und Vererbungslehre.*

By Hermann Ullrich and August Arnold. Based on the "Grundriß der allgemeinen Botanik," by Karl Wetzel. Walter de Gruyter & Company, Berlin. DM.28.50. xvi + 424 pp.; ill. 1953.

This new and profusely illustrated textbook of general botany follows established lines of presentation in the delimitation and arrangement of the major botanical sciences, but contains a large amount of new material not customarily included in general textbooks. A short review of the plant kingdom using a conservative classification precedes the main part of the book. The latter opens with a chapter on cytology which is followed by another on histology. The bulk of the book (pp. 81–352) deals with morphology, the vegetative and reproductive phases being treated separately. A short chapter on resting stages and life-forms of flowering plants concludes this part of the book. The final chapter covers the field of genetics in greater detail than is done in most textbooks of general botany. The numerous illustrations are well selected from traditional and modern sources, as is evident from the detailed source list given at the end of the book. It is hoped that Volume Two will be a fitting companion, thus making this work an important addition to the textbook literature of general botany.

THEODOR JUST

LEHRBUCH DER BOTANIK *für Hochschulen. 25th Revised Edition.*

By Hans Filling, Walter Schumacher, Richard Harder, and Franz Firbas. Piscator-Verlag, Stuttgart. DM. 24.00. xii + 626 pp.; ill. 1951.

The "Viermännerbuch" is one of the best known textbooks of botany, the first German edition having been published in 1894 and 16 editions in foreign languages having followed at various times. Six of these were in English, the last one having appeared in 1930 as Strasburger's *Text-book of Botany*. Although the original authors have long been dead, others have successfully carried on the tradition and kept the book up to date. Throughout all these years, the basic plan of the work has never been changed, although in 1939 a new section on ecology and plant distribution by Franz Firbas was added as a permanent feature. The colored map, showing the distribution of the major vegetation types and floral regions of the world, is based on similar maps by Brockmann-Jerosch and Diels and represents a valuable feature not found in other textbooks of botany. As the previous edition was issued in 1939, the authors have attempted to revise the text by adding much new information, indicative of the many advances made during the intervening years. Formerly published by Gustav Fischer, Jena, the rights have by now apparently been transferred to a new publisher in Stuttgart. Unfortunately, post-war paper and printing standards cannot yet match the excellent workmanship seen in the last prewar edition. Despite these minor physical

shortcomings, this classic textbook is bound to win many new friends at home as well as abroad.

THEODOR JUST

GRUNDLAGEN UND METHODEN EINER ERNEUERUNG DER SYSTEMATIK DER HÖHEREN PFLANZEN. *Die Forderung dynamischer Systematik im Bereiche der Blutenpflanzen.*

By Franz Buxbaum. Springer-Verlag, Wien. $6.20 (paper). xi + 224 pp.; ill. 1951.

Although the literature pertaining to the "new systematics" is rapidly growing to sizable proportions, this book represents a timely and provocative addition to it. For in the opinion of the author, the purely descriptive and essentially static part of taxonomy dealing mainly with infraspecific and specific levels differs profoundly from the truly systematic (phylogenetic) and dynamic part of taxonomy that is largely concerned with the definition, relationships, and evolution of higher categories (genera, etc.). As the taxonomic methods used for the study of species and their constituent populations are well enough known and treated in available textbooks and monographs, the author devotes the greater part of his book to a discussion of the theoretical and practical methods and concepts used in studying higher categories. Examples, often based on the author's own researches, are used to illustrate his methods. By doing so, the reader is effectively introduced to the workshop of the practicing taxonomist. This alone would suffice to distinguish Buxbaum's book and views from others in the field. As most genera were described long ago, restudy of their constituent species may bring about their redefinition and reclassification as well as that of related genera. Although written for use in taxonomic work with flowering plants, the same methods and concepts can well be applied, *mutatis mutandis*, to other plant groups.

THEODOR JUST

GRAY'S MANUAL OF BOTANY. *Eighth (Centennial) Edition.*

By Merritt Lyndon Fernald, with assistance of specialists in some groups. American Book Company, New York. $9.50. lxiv + 1632 pp.; ill. 1950.

In the United States few botanical books have been expected with more anticipation and interest than has the eighth edition of *Gray's Manual of Botany*. Fortunately, it appeared before the death of Professor Fernald, who had devoted most of his life to revising the seventh edition (1908). While the latter contained descriptions of 4,885 species, varieties, and named forms, more than 8,000 entities are treated in the Centennial Edition. This increase in contents has necessitated an increase in the number of pages from 926 to 1,682, as well as a larger format. The area covered was also extended beyond the old *Manual's* range. These major changes required almost complete reworking and rewriting of most descriptions, keys, distribution data, and comments. The scientific names used are in accord with the International Rules of Botanical Nomenclature. As in the last edition, the system of classification proposed by Engler has been followed. In addition to the assistance received from some 400 correspondents, Fernald derived aid from detailed studies published by others and included several sections contributed by specialists. Although botanists may not always share the opinions of the author regarding the definition, status, and names accorded certain entities, and although some descriptions may better fit material collected in New England than in the marginal parts of the *Manual's* range, the Centennial Edition of *Gray's Manual of Botany* is bound to remain the authoritative work on the plants of the Central and Northeastern United States and Adjacent Canada.

THEODOR JUST

RESEARCHES ON FUNGI. *Volume VII: The Sexual Process in the Uredinales.*

By the late A. H. Reginald Buller. University of Toronto Press, Toronto. $13.50. xx + 458 pp.; ill. 1950.

The book is a detailed account of the course of development, structure, and function of the organs involved in sexual reproduction in the rust fungi. It begins with a thorough history of the study devoted to this aspect of the Uredinales, and follows with descriptions of the process of sexual reproduction in many of the common and some of the uncommon rust fungi. A brief concluding chapter presents the author's views on the possible evolution of the association of rust fungi (and certain other fungi) with insects. The major portion of the material presented in the book is derived from the personal researches of the author and that of his associates and students, but there are abundant references to the works of others who have contributed information—or, at times, misinformation—to this subject. The emphasis throughout the work is on fresh, living material. There are 124 illustrations, both half-tones and line drawings, ranging from good to excellent in quality. All of them contribute to the reader's understanding of the material presented. Specific literature citations are carried at the foot of the page. This adds greatly to the usability of the book.

The book is scholarly and informative, but also readable, interesting, and often fascinating. It should be indispensable for professional workers in plant pathology, mycology, and general botany. Those in

other fields who might be interested in sexuality in the microorganisms, or in the general biology of sex might acquire from this book an increased understanding of this aspect of the supposedly simple fungi. Research workers in fields not at all related to mycology might find the book of value for what it reveals in the way of productive methods of attack on difficult research problems.

CLYDE M. CHRISTENSEN

NATIVE ORCHIDS OF NORTH AMERICA—*North of Mexico. Vol. XXVI. A New Series of Plant Science Books edited by Frans Verdoorn.*

By Donovan Stewart Correll; illustrations by Blanche Ames Ames and Gordon Winston Dillon; cultural notes by Edgar Theodore Wherry and John Vertrees Watkins; foreword by Charles Schweinfurth. Chronica Botanica Co.; Stechert-Hafner, New York. $7.50. xvi + 399 pp.; ill. 1950.

Although the literature dealing with orchids is vast and costly, much of it is concerned with exotic members or horticulturally important groups rather than with species found in temperate regions. It is indeed refreshing that the author has prepared the first complete account of all orchids found to date in North America north of Mexico. As the book is scientifically accurate and eminently readable, it should have a wide and genuine appeal. In view of the fact that the orchid family, containing more than 15,000 species, is possibly the largest family of flowering plants, the North American orchid flora appears to be comparatively small, consisting of but 46 genera with nearly 200 species and variants. Of these, 110 entities are known from the southeastern United States. The treatment of each entity includes its scientific name, synonyms, common names, description, geographical distribution, comments, frequently cultural notes for those who like to grow native orchids, and in most cases attractive habit sketches. Having travelled widely in the United States and Canada, the author has recorded many personal collecting experiences, as in the case of the Northern Coral-root (*Corallorhiza trifida* Chat.) collected by him in British Columbia along the Alaskan Highway. That collecting orchids in North America may still yield its surprises is shown, for instance, by the discovery in 1937 of *Maxillaria crassifolia* (Lindl.) Reichb. f. in the Big Cypress Swamp of southern peninsular Florida, "the haunt of many of the interesting orchids found in Florida." For the convenience of the reader, a general introduction presents the main characteristics of the family and other useful information. A glossary of the terms used, a list of abbreviations of authors' names, and a selected bibliography are appended. Good indexes to the scientific and common names conclude this volume. This beautifully illustrated book is convincing evidence of what can be done in presenting botanical material in a scientifically sound yet pleasing manner for the benefit of botanists and orchid lovers in general.

THEODOR JUST

CATALOGUE DE LA FLORE DE L'OUBANGUI-CHARI. *Mémoires de l'Institut d'Études Centrafricaines, No. 2.*

By P. Ch. Tisserant. Institut d'Études Centrafricaines, Brazzaville (A.E.F.). 800 fr. (paper). 165 pp. 1950.

The African flora is one of the great floras of the world and is by no means well-known. Its broad outlines have been sketched by the series of regional floras issued by the British working at Kew Gardens. The more intensive field work essential to a real inventory of this flora is being done by hundreds of visiting and local botanists, such as the author of this catalogue.

This work, obviously a very preliminary annotated list, embodies the results of years of patient collecting of specimens and of detailed information on the occurrence, native uses, and especially native names of African plants, as well as taxonomic study both by the author and by specialists to whom specimens were sent by him. The families of angiosperms are arranged alphabetically, with the genera alphabetical under each. Within a genus the species for which special information is avaiable are listed first. These may have native names in several dialects, notes on the ecology and outstanding features, and finally ethnobotanical material on the place of the species in the native culture. Following these better-known species are lists of additional species, identified or not, which the author has collected. Finally there are listed still other species previously recorded from the region by A. Chevalier.

The area covered is an enormous one, lying between the Cameroons and the Anglo-Egyptian Sudan and between Chad and the Belgian Congo, mostly north of the limits of the great equatorial forest. A sketch map is provided, showing the principal rivers, towns, the limits of the dense forest, and the regions occupied by the largest native tribes. There is no geographical description or discussion whatsoever, nor any description of the vegetation. The botany suffers from a lack of acquaintance with many modern studies and from a rather casual attitude toward correct botanical nomenclature. The most serious criticism of this work, though, is of the lack of any actual citation of herbarium specimens, preserved as vouchers for the names and observations recorded. Had such citations been given, the work would have permanent value regardless of the quality of the identifications and nomenclature. If specimens were mentioned by collector and number, with a record of the herbaria where they are deposited, the work would remain permanently verifiable.

On the credit side, a very critical discussion of native names is provided. The traps and pitfalls besetting the collector of these names are described so adequately that this part of the introduction should be required reading for every botanist and ethnobotanist setting forth to gather such information. Those of us who have collected native names in the past are sorely tempted to discard the whole lot after reading this. The author obviously knows the psychology of his informants very well.

This treatise is scarcely to be recommended as a means of identification of, or guide to, the plants of this section of Equatorial Africa. It may often confuse the user unless he understands its preliminary nature and limitations. But it is from such information as that recorded here that more adequate and critical local and regional floras are built. For the field worker it is a great convenience to have an easy means of telling whether particular data are already on record or are new and worth taking down. For the anthropologist and ethnobotanist the placing on record of such data is an urgent task that cannot be postponed, With every old native that dies some of this lore is lost. It is indeed fortunate that in this region of French Africa there is a student who combines a botanical background with such enthusiasm for native lore as to record the ethnobotany of these relatively primitive peoples. His scientific colleagues in both botany and ethnology do well to encourage him to carry on this work and to publish his findings.

F. R. FOSBERG

THE GENERA OF SOUTH AFRICAN FLOWERING PLANTS. *Second Edition.*

By E. Percy Phillips. Government Printer, Pretoria. £2. vii + 923 pp. 1951.

The exceeding richness of the South African flora, famed among botanists since Thunberg, pupil of Linnaeus, first studied it in the late 18th century, is amply brought out by this great book. In it are fully described almost 1800 genera of flowering plants, 142 more than were included in the first edition, 25 years ago.

The work consists strictly of descriptions and keys to families and genera, with brief statements for each of total number of genera (in families) and species (in genera), geographical range, number of species in South Africa, whether native or introduced, a brief statement of South African range, with mention of the type species. The only introductory material is a 3-page discussion of the benefits of assembling private herbaria and directions for preparing them. The descriptions are quite standardized and comparable, and are based almost entirely on the author's own examinations of the plants. As a consequence of this, they frequently fit the South African species rather than the whole genus. Often this is acknowledged to be the case.

Artificial keys of a numbered bracket type are provided to families and to the genera within each family. These, in general, are quite workable, though some weak places are readily noted. That such should not occur would be much more surprising than that they do in a work of this magnitude.

The treatment of genera is generally rather conservative, at least in families well known to the reviewer. This is not true of the Aizoaceae, in which the author admits 146 genera, but with strong misgivings and doubts. Probably each specialist will note places in the families that he knows best where the treatment could be improved or more modern views adopted. This is also to be expected in a work of such size and character.

The importance of this book is greatest, of course, to South African botanists and to those who must deal with plants from that part of the world. To them its publication must, indeed, be a major event. But it is by no means devoid of interest to others. Examination of the statements of ranges and numbers of species and their comparison with those of genera in other parts of the world will yield much of importance to floristic plant geographers. Furthermore, this material seems to be much more carefully compiled than is usual. The number of endemic genera, the number of genera from other parts of the world with one or two of a few South African species, and the number of genera common to Africa and America will surprise many botanists.

Perhaps the greatest importance of this book to those in other parts of the world might be to serve as an example. A treatment of the genera may well be the single botanical work most needed in almost all of the major regions of the earth. Phillips has shown that such a work is a practical accomplishment, even for one man, and has provided a first-class model.

F. R. FOSBERG

PLANT PHYSIOLOGY

PLANT PHYSIOLOGY. *Second Edition.*

By Bernard S. Meyer and Donald B. Anderson. D. Van Nostrand Company, Toronto, New York, and London. $8.50. viii + 784 pp.; ill. 1952.

During the 14 years that have elapsed since the appearance of the first edition of this book (*Q.R.B.*, 15: 98. 1939), much has happened in the science of plant physiology. New experimental methods have been developed, advances have been made in biochemical knowledge, isotopes have been applied to many basic problems, and a revolution in agricultural practice has occurred through the use of plant hormones. All of these events and others are faithfully recorded in this

second edition. The authors, however, have not been so successful in keeping up with the great changes that have taken place in the teaching of plant physiology. With the trend toward biology divisions and general biology courses, plant physiology is now generally taught as a one-semester elective course during alternate years. Although the authors of this textbook claim that it can be adapted to a one-semester or one-quarter introductory course, it will require considerably more effort and time than the teacher can spare to fit this book to the needs of such a class. The net result will be that the students will be dragged through approximately 300 pages dealing with water relations before they get into the interesting part of plant physiology. By that time the semester will be over. The student that survives this treatment and goes on to advanced work in plant physiology deserves a gold plated atmometer (see p. 128 for a drawing).

The book contains a wealth of sound information necessary as background material for anyone who is going into the field of plant physiology. I feel, however, that the value of the book suffers because of its organization. I do not doubt the importance of a study of water relations, but any extended discussion should be attempted at a more mature level. The basic information on photosynthesis is given, but how much more thrilling would have been the unfolding of a 200-year-old mystery which even today is one of the most active fields of research. The same is true for the material on plant hormones. The basic information is all there, but nowhere is the student informed of the part that plant physiologists are playing in the development of one of the biggest agricultural businesses in the world. The authors may feel that it is the duty of the teacher to bring out the historical development of plant physiology; but I, for one, feel that a work to be used as an introductory textbook should bring out these other features. As for the book itself, some recent material has been added, some sections have been reorganized, and the book costs about twice as much as the first edition.

G. R. NOGGLE

ECONOMIC BOTANY

POISONOUS PLANTS OF THE UNITED STATES. *Revised Edition.*

By Walter Conrad Muenscher. The Macmillan Company, New York. $4.00. xviii + 277 pp.; ill. 1951.

Muenscher's well-known work was first published in 1939 (*Q.R.B.*, 14: 364. 1939). The present volume incorporates the additions to our knowledge of vascular poisonous plants growing in the United States that have accumulated in the ensuing decade. About 25 more species are included, and several of the topics have been revised and amplified. The original organization and treatment of the subject have been preserved in the current edition: arrangement of the plants is by families, and in general the discussion of each plant includes name, description, distribution and habitat, poisonous principle, conditions of poisoning, and symptoms. In some cases simple remedies are indicated, or references to treatment are cited. The book is well indexed and is illustrated with 83 line drawings.

SCOTT S. PAULEY

PLANTS FOR MAN.

By Robert W. Schery. Prentice-Hall, New York. $7.50. viii + 564 pp.; ill. 1952.

While it is obvious to most that man is still very much dependent upon plant products for his everyday existence, it took the deprivations of World War II to bring forcibly to the forefront the fact that no country can supply its entire needs as regards plant products. The scramble for natural rubber, when the Malayan peninsula was overrun by the Japanese, is a case in point. The search for new sources of quinine was another. Yet these two substances are but a few of the thousands of materials derived from the floras of the world which supply man with some of his needs. Apart from the foods we use, there are innumerable uses to which the woods, fibers, exudates, oils, tannins, fats, and carbohydrates are put in industry, medicine, and agriculture. Along with the discovery and development of these uses has come the exploitation of the product, the struggle for control, and in some instances the decline of need for the natural product as synthetic means of production have supplemented or supplanted the natural source. In their total, these plants and their uses, together with whatever history goes along with them, make up what we call "economic botany," and it is with this field that *Plants for Man* is concerned. The volume can be called a textbook, for it is essentially that. It is, however, much more, for the author has a feel for the fascination of his subject and an ability to convey this in a highly readable style. The result is a book which should be required reading for any student in biology. In fact, this volume could replace a good deal of the material generally given in the usual beginning botany course, and the student would find it a far more interesting aspect of that branch of biology than much of what passes for botany. The author is to be commended for an excellent book.

C. P. SWANSON

PAGEANT OF THE ROSE.

By Jean Gordon. Studio Publications in association

with Thomas Y. Cromwell Company, New York. $5.00. 232 pp.; ill. 1953.

Symbolically the rose has figured in romance, art, legend, and religion more than any other flower. Universal in its appeal, it has been claimed by almost every culture. It was the floral symbol of Venus, it arose from the ashes of the Hebrew maiden Zillah as well as from the ashes of the fire intended to consume the Persian prophet Zoroaster, it figures in the legends of the Mohammedans, Christians, and even the Cherokee Indians, and it has served as a symbol for various warring factions. It is with these things that the author has dealt. The result is an intriguing volume which the rose lover can well place beside other volumes of a more horticultural and more practical nature.

C. P. SWANSON

THE STORY OF TOBACCO IN AMERICA.

By Joseph C. Robert. Alfred A. Knopf, New York. $5.00. xxxvii + 296 pp. + 4 pl.; text ill. 1952.

Tobacco is many things to many men: cash crop, exhauster of soils, staple commodity of commerce, the economic basis of some of the Colonial governments, as well as the source of smoking and chewing pleasure to widespread races of mankind. Its place in history and in industry is as well established as its place in society, and probably more than any single plant species it has a personality which sets it quite apart from its brothers. It is of the historical and the social aspects of the plant that J. C. Robert writes, and he tells his tale with enthusiasm, with humor, and with a sense of the dramatic, at the same time that he retains the authority of the trained historian. Distinct from the medicinal aspects of tobacco, the historical role of the plant in colonial life revolved around, and helped to shape, the economics of the southeastern states. Its influence on slavery, on commerce and diplomacy with England and with the northern colonial states, and on the wars with England stemmed from the fact that it was one of the great cash crops upon which the social system of the south depended. Anything that impinged upon or interfered with, the ultimate profit from tobacco was of both national and regional importance. Like cotton, it helped to mold the South. This makes a story well worth reading.

C. P. SWANSON

PATHOLOGY IN FOREST PRACTICE. *Second Edition.*

By Dow Vawter Baxter. John Wiley & Sons, New York; Chapman & Hall, London. $7.50. xiv + 601 pp. + 1 pl.; text ill. 1952.

The appearance of a second edition of *Pathology in Forest Practice* within 10 years is proof that the author's method of presenting forest pathology to the student with "emphasis on the crop and product" is well received. To those readers who are not familiar with the first edition, it is a volume prepared primarily for the student and the timber grower and user. The author emphasizes the importance of site and cultural practices in dealing with tree diseases.

There have been no fundamental changes in the presentation of material. The discussions of some diseases have been appropriately moved from one chapter to another, e.g., anthracnose of sycamore from the chapter on plantation diseases to that on shade and park trees. The discussion on needle blight of Douglas fir was moved in like manner, but why is not apparent. Such of the less important forest disease problems as twig blight of Asiatic chestnut, root rot of rubber, and also a discussion of poisonous mushrooms have been wisely omitted from the second edition so as to allow the inclusion of data on new diseases and recent information on important older diseases without increasing the size of the volume. Some of the new or expanded topics are pole blight of Western white pine, littleleaf of Southern pines, dieback of birch in New England, oak wilt, phloem necrosis of elm, the use of chemical preservatives in wood products, damage from chemical weed killers in nurseries, and the role of aerial surveys in damage appraisal studies.

The presentation of the material makes it quite difficult to obtain a complete story of any disease that happens to affect trees of various ages. For example, various phases of the fusiform rust disease are mentioned in at least 6 different places in the book, the causal fungus in at least 9 different places. Errors are rare (but note the transposed photographs in Fig. 7). Less obvious but more misleading are errors of the type seen in the discussion of the brown spot needle disease of pine where the author states "In the spring following initial infection, fruit bodies appear on the brown spots of the diseased and dead needles," whereas fruit bodies actually appear at all seasons; and again in connection with the same disease, "Grass in longleaf-pine plantations offers much competition and it favors the incidence of brown-spot disease," whereas published information shows the reverse to be true in regard to this disease. There seems to be no regularity in giving the authors of scientific names of the disease fungi, e.g., *Phytophthora cactorum* is mentioned at least 4 times with the authority given 3 times and the abbreviations of the authors' names different in each case. The second edition, like the first, has good text figures and adequate reference lists at the ends of chapters.

BERCH W. HENRY

AMERICAN FOREST POLICY. *A Study of Government Administration and Economic Control.*

By Luther Halsey Gulick. Duell, Sloan & Pearce, New York, for the Institute of Public Administration. $3.50. 252 pp. 1951.

The settlement and political consolidation of a continental empire of almost 3 million square miles in little more than a century is, as Gulick points out, an exploit without parallel in human history. The primary government policy responsible for this remarkable feat was that of rapid land distribution in small parcels to private owners. Although a highly successful settlement technique, this policy has created a serious timber problem for our own time and for the future. To the early settler, timber was a nuisance, and in some cases a more formidable enemy than the native Indian inhabitants. His principal struggle was directed against the trees in order to open the country for the establishment of safe homes, farming land, and roads. Later, with wood in demand, much of the remaining accessible timber was cut and sold. Finally, with the continuing depletion of the forests, there grew the feat of timber famine; and by the latter part of the 19th century, a conservation movement had started. Patterned closely after the forest policies of Western Europe, this conservation program had as its central point the advocacy of positive governmental action. Such a program has now been in operation for about 40 years in the United States, and the objective of Gulick's book is to answer the questions: "In the area of forestry, how has the government sought to influence the economy, through what devices, with what results in administration and to the economy?" Written with apparent objectivity and certainly with great lucidity, this book may be read with interest and profit by all biologists, regardless of their special field.

SCOTT S. PAULEY

WOOD TECHNOLOGY. *Constitution, Properties, and Uses. Third Edition.*

By Harry Donald Tiemann. Pitman Publishing Corporation, New York, Toronto, and London. $6.00. xvii + 396 pp. + folded chart; ill. 1951.

The first edition of *Wood Technology* was published in 1942, and the second two years later. Due to wartime restrictions on the use of paper, considerable abridgment was made in the 1944 edition. In the present edition, however, deleted topics have been restored, and amplified by the addition of new material. The primary objective of this work is to present a complete survey of the field of wood technology in such a way as to be understandable to non-technical, as well as to scientific, readers. Such treatment has of necessity required much condensation of certain topics, but this detracts in no way from the basic usefulness of the book. Selected references are listed at the end of each chapter for the convenience of the reader interested in a more detailed treatment of the subject. The book is well illustrated by the use of numerous half-tone photomicrographs, line drawings, graphs, and tables.

SCOTT S. PAULEY

GENERAL AND SYSTEMATIC ZOOLOGY

METHODS AND PRINCIPLES OF SYSTEMATIC ZOOLOGY. *By Ernst Mayr, E. Gorton Linsley, and Robert L. Usinger. McGraw-Hill Book Company, New York, Toronto, and London.* $6.00. x + 328 pp.; ill. 1953.

The scope and organization of this book are well set forth in its chapter headings, in which taxonomy is throughout synonymous with systematics. Three chapters compose a section on taxonomic categories and concepts, namely: Taxonomy, its History and Functions; The Species and Infraspecific Categories; and Classification and the Higher Categories. Six chapters are devoted to taxonomic procedure. These are: Collecting and Collections; Identification and Taxonomic Discrimination; Taxonomic Characters; Quantitative Methods of Analysis; Presentation of Findings; and Preparation of Taxonomic Papers. The 8 chapters of Part 3 compose a review and discussion of zoological nomenclature. The several chapters constitute thorough and competent discussions of their subjects.

Nevertheless, I find this book extremely difficult to review objectively. Its subject matter is so much within my field of interest that divergence from my own views and prejudices, and omissions of ideas familiar to me, loom disproportionately large. I would have written a different book, and not having written one makes my criticism of an existing work seem invidious. The terms "habitus" and "heritage," and "paleotelic" and "neotelic," with which I grew up, are not mentioned at all. I do not find the expanding species adequately contrasted with the declining one. Fundamentally, I find that the book lacks perspective, even historical perspective, and that it lacks humor. A few anecdotes, like the story explaining how T. D. A. Cockerell came to write more than 5000 scientific papers (Cockerell is mentioned anyway), would have lightened a book that is rather heavy going at best.

There is only a single clue (p. 9) to the fact that among the subsciences of biology, systematics is the Cinderella. Systematic zoology and botany are sadly in disrepute as fields of research in the majority of American universities and colleges, and indeed in those of the rest of the world, and this is a situation of long standing. Demonstration of the merits of systematics and of the need for its restoration to the respected categories of learning within the structure of higher education should be one of the major objectives of a general account of taxonomy. A chapter setting forth the somewhat curious history of the decline of system-

atics, a chapter not particularly creditable to the biological leaders involved on either side, would have been illuminating and significant.

The temptation to comment on the successive chapters, balancing large praise against small blame, is great. It would be easy to expand this review unduly, and I shall limit my further remarks to the section on nomenclature and to the chapter on the preparation of taxonomic papers.

Zoological nomenclature is intelligently and critically discussed. For this section, as for the book as a whole, I should have liked a more extended introduction. That nomenclature should be the handmaiden of taxonomy, not the mistress (as the botanists say) is less solemn than that nomenclature is a means to an end, and not an end in itself. These phrases state the most fundamental principle of nomenclature. Mayr, especially, has taken a lead in criticism of the system of nomenclature that gives lip service to the ideal of stability but engages actually in a complicated game of changing the scientific names of the best known of animals. This is the essence of fifty years of operation under an international code of nomenclature.

The proposal of new names, of which the author's own name forms a part, has so obvious an appeal to personal vanity that with the establishment of an elaborate and intricate set of rules the naming of animals and the disentanglement of the nomenclatural tangles that have resulted from past naming combine to become a game of an extraordinary kind, with a most potent appeal to esoteric scholarship. To play this game well, one needs acquaintance, at the least, with Greek and Latin and with a variety of modern languages. When such scholarly equipment is reenforced by a legalistic turn of mind, the game may become so fascinating as to seduce even a great zoologist. One's colleagues become one's rivals, and the race for priority may affect the gathering of specimens in remote parts of the world, and the nocturnal operation of the printing press. The prize to be gained is the satisfaction of vanity; and the appearance of an advancement of science is fallacious. The descriptions of most of the species named during the past century require complete reworking in order to make them useful to a modern conspectus of the animal kingdom.

Not to recognize that nomenclature has this aspect of a game, and to confuse zoological nomenclature with systematic zoology, is to lose one's perspective and one's sense of humor. Perspective and humor seem to have been and still to be singularly lacking among nomenclaturists, and, I regret to say, among systematists. Those who want time for the problems of zoology must avoid entanglement in nomenclatural pseudo-problems. Failure to take up the Alexandrian sword means retreat into a Gordian web of argument and resolvement of argument.

A chapter on the preparation of taxonomic papers could not fail to intrigue a taxonomist who has dealt with many editors and been an editor in his turn. Much of this chapter is unexceptionable as orientation for the would-be writer of scientific papers, but there is too much of detail and not enough of principle. In the first place, one does not set out cold to write a taxonomic paper; one has had the papers of one's colleagues and predecessors and mentors as models. Judgment is needed to choose between good papers and bad. Parenthetically, the book fails to deal adequately with the curse of bad taxonomy, which is doubled and redoubled by the requirement that good taxonomy take account of the bad. The urgency of conciseness, in face of the growing volume of scientific literature to be reviewed by student and researcher, is not sufficiently emphasized. Sir Ernest Gower's *Plain Words* and the indispensible *Fowler* are better guides (and afford better warnings) than the manuals of style referred to, which are deficient in wit. *Plain Words* contains the original quotation, from a child's essay, of the now widely quoted description of the blind owl and the six-sided cow. The most important item in the revision of a manuscript in my editorial experience is to choose words for exactness of meaning (or for closeness of approximation to meaning) instead of using any old words that pop into the writer's head. I have spent more time than I wish to remember translating what authors have written into what they meant. The chapter has no hint to the young author as to how to deal with, or cultivate, editors. For a chapter on the preparation of papers, I find an undue number of superfluous phrases and of misused words. Finally, the student should be assured that the attainment of the reasonably accurate style essential to scientific writing is worth the years of effort and practice that may be required.

Methods and Principles of Systematic Zoology is a useful work, even an indispensable one, for the working systematist at every level. It may well play an important part in the current rejuvenation of systematics and the restoration of systematists to scientific respectability, a movement now actively supported by such sister sciences as genetics and ecology.

KARL P. SCHMIDT

FRESH-WATER INVERTEBRATES OF THE UNITED STATES. *By Robert W. Pennak. The Ronald Press Company, New York.* $14.00. ix + 769 pp.; ill. 1953.

In this day of specialization and compartmentalization such a single-handed work as this is rare, and the author has earned our admiration for completing this formidable, self-assigned task. It must also be remembered that while this book is designed to serve as a handbook as well as a text for advanced and undergraduates and beginning graduate students, it was not intended to be a complete guide to all the fresh-water invertebrates in the United States. It is essentially an eclectic handbook, with keys to the genera, and includes

the most common, characteristic, or interesting species. In only a few groups are all the known species included, which means that specialists in those groups not completely treated will probably question the author's choice of species or his literature selections. Nevertheless, such a book has long been needed, and it will probably become a familiar companion for summer field courses.

The book opens with an introductory chapter dealing with the ecological principles affecting life in fresh waters, and descriptions of the various habitats, including other types of inland waters. Here a table of the salt composition of such inland waters, as well as of "hard" and "soft" waters, should have been included; the student will have to seek such information elsewhere. Chapters 2 to 37 are built on an identical basic plan: first, an introductory description of the morphology, ecology, physiology, and taxonomy of the group, with methods for collecting, preservation, and study, followed by an illustrated key. More often than not this key is to genera, although in many of the smaller or better known groups the key goes to species. It is unfortunate that the keys were printed without "kick-backs," i.e., numbers in parentheses after the principal number to indicate the previous dichotomy. Without such a mechanical convenience, it is easy to become lost in a key that has more than 25 or 30 divisions—and the present key to the protozoa has 307! This is an oversight that cannot be easily corrected in a second edition without extensive typesetting, and the highly glazed paper stock makes it difficult to write such annotations on the margin, even though it shows off the photographs and drawings nicely. (This paper stock also causes the book to weigh slightly more than four pounds!)

In a work of this scope errors in detail are inevitable, but a list of those detected by the reviewer would serve only to show off his particular fields of specialization. It does not appear, however, that the possibility has been considered that some of the so-called relict species which have marine relatives may actually be ecotypes of the marine species. The section on the geographic distribution and dispersal of the decapod crustacea is concerned principally with crayfishes, and no mention is made of the interesting problems posed by the distribution of freshwater shrimps in the United States.

The general sections on the various animal groups are on the whole adequate summaries, although those on groups more abundant in the sea might have been given more detailed treatment for the benefit of inland zoologists. This is particularly true of the molluscs. The section on freshwater lamellibranchs needs a brief discussion of their relation to the marine forms, and in the section on the gastropods some consideration of functional adaptation in the various orders would have enhanced the discussion. There are appendices on apparatus and reagents, and a detailed index.

JOEL W. HEDGPETH

THE "MANIHINE" EXPEDITION TO THE GULF OF AQABA, 1948–1949. *Bull. Brit. Mus. (Nat. Hist.) Zoology, Vol. 1, No. 8.*

British Museum (Natural History) London. 25s. Pp. 153–252 + 11 pl.; text ill. 1952.

The Gulf of Aqaba is a part of the Red Sea judged particularly interesting, since it is well cut off from an influx of faunal elements from the Arabian and Mediterranean Seas and therefore may be supposed to contain a large percentage of endemic animals. Following brief reports on the stations of collection and the hydrological conditions in the Gulf, the major part of the present publication is devoted to taxonomic reports on the material collected. Sponges are treated by M. Burton, the polyclad Turbellaria by S. Prudhoe, Gephyrea by A. C. Stephen, Mollusca by W. J. Rees and A. Stuckey, Echinodermata by A. M. Clark (still another Clark in echinoderms!), Tunicata by W. G. Van Name, and fishes by N. B. Marshall. In the account of the Mollusca, most attention is paid to cephalopods; and there are valuable data of measurements. There is a considerable number of echinoderms in the material and the usual preponderance of fishes, 113 species. Scarcely any new species appear. In regard to fishes, some evolution of endemic elements as a result of isolation in a partially inclosed area is indicated.

L. H. HYMAN

MADREPORARIA, HYDROCORALLINEA, HELIOPORA AND TUBIPORA. *Great Barrier Reef Expedition, 1928–29. Scientific Reports, Vol. VI, No. 31.*

By Cyril Crossland. British Museum (Natural History) London. £3 10s. Pp. 86–257 + 56 pl.; text ill. 1952.

This is stated to be the largest collection of corals from any one area, comprising 174 species, of which 32 are new. The account is purely taxonomic and is abundantly illustrated with splendid photographs of the specimens.

L. H. HYMAN

A REVIEW, AND A REVISION IN GREATER PART, OF THE CTENISCINI OF THE OLD WORLD (HYM., ICHNEUMONIDAE). *Entomology, The Brit. Mus. (Nat. Hist.,) Vol. II, No. 6.*

By G. J. Kerrich. The British Museum (Natural History) London. 30s. (paper). Pp. 305–460 + 4 pl.; text ill. 1952.

So far as known, tryphonine ichneumon flies of the tribe Cteniscini are parasites of sawfly larvae, especially of diprionids and nematinids. Kerrich's monograph aims primarily at a taxonomic revision of European forms. In addition to the keys and characterizations of

the tribe, genera and species, it includes notes on new and known host relations of cteniscinids, a table of the occurrence of 46 species in Ireland, England, northwestern Europe, Sweden and Finland, and a very interesting flight-table of 38 European species. In all, 8 genera and 56 species are described, of which 2 genera (*Parexyston* and *Eudiaborus*) and 10 species are new.

Of the Old World forms, species of *Acrotomus* s. str., *Diaborus auctt.*, red-bodied *Cteniscus* (the black-bodied species being given very thorough treatment by Kerrich), and males of *Exenterus*, still await intensive study. The 4 illustrations of cteniscinids that are reproduced as full-page plates are admirable, and fittingly conclude this study that contains much new information.

KENNETH W. COOPER

A HISTORICAL REVIEW OF THE MOLLUSKS OF LINNAEUS. *Part 1. The Classes Loricata and Pelecypoda. Bull. Amer. Mus. Nat. Hist., Vol. 100, Art. 1.*

By Henry Dodge. American Museum of Natural History, New York. $4.00 (paper). 263 pp. 1952.

The publication of the tenth edition of the *Systema Naturae* of Linnaeus occurred nearly two centuries ago. One would think that after the passage of such an interval there would remain no information that had not already been gleaned from the treatise, but such is not the case. Like the orange which Patsy presented to Miss Wiggin, "it has been skwuz some, but there is some juice left in it."

When Linnaeus wrote there were only about 40 recognized genera of the Mollusca. It would be a bold man who would attempt to estimate the number generally recognized today, but there must be roughly several hundred times as many. The old genera had to be broken up, not altogether because they would have become too large for comfort, but because they were for the most part miscellaneous aggregations of species not especially closely related.

The number of recognized species has increased proportionately, with the result that many of the brief diagnostic phrases used to characterize the Linnaean species, although quite adequate at the time when proposed, no longer suffice even for the discrimination of genera. Consequently the decision as to which modern species shall bear the original Linnaean name is often one of considerable complexity, necessitating examination of the original material in many cases. Unfortunately, this is often not possible.

In the present work the author has made identifications of all the Linnaean species of the Amphineura and Pelecypoda. The task has involved the examination of material in museums and of various publications of the last three centuries as well. Anyone engaged in work on molluscan systematics will welcome this book, and will look forward to the appearance of future instalments of it dealing with the Scaphopoda, the Gastropoda, and the Cephalopoda.

JOSHUA L. BAILEY, JR.

THE FEEDING MECHANISMS OF A DEEP SEA FISH. *Chauliodus sloani Schneider.*

By the late V. V. Tchernavin. British Museum (Natural History) London. £1 10s. viii + 101 pp. + 10 pl.; ill. 1953.

This paper is a worthy valediction of the late eminent Russian scientist, Tchernavin. Included are a brief biography and bibliography of the author.

As long ago as 1918, Tchernavin published a paper on the breeding changes in the skeleton of a salmon, and Part I of the present thesis is devoted to the Mechanical Working of the Head of Salmon when Catching and Swallowing Large Prey. This is followed by the main body of the paper, treating of the same activities in *Chauliodus*.

The general presentation is excellent, the dictum succinct, clear, and balanced, and the author's illustrations show scrupulous accuracy and craftsmanship.

The complexity of the intermovements of the various cephalic elements in this fish in the operation of swallowing makes it impossible to summarize them in a brief review. In short, the process is accomplished by the expansion of the pharyngo-oral cavity, the widely opened mouth, combined with the downward pull of the shoulder girdle, drawing with it out of the way the ventral parts of the branchial arches, the heart, and the ventral aorta. Understanding of the operation is aided by a series of illustrations showing the passage of a fish into the mouth and down the gullet.

Treatment of the swallowing of small prey includes the location, description, and supposed uses of the several thousand light organs. Among these uses are the illumination of the surrounding water and approaching prey, attracting prey in front of the mouth and guiding them directly into the mouth cavity, and sex recognition.

It is a great pity that the material on which the author worked was all dead and preserved. The sight of a freshly caught, living *Chauliodus* giving out brilliant colors of bluish-violet, coral, amethyst set in gold frames, and bright purple will never be forgotten. I am inclined to question the assumption of the luminosity of the minute enlargement on the tip of the elongated, second dorsal ray. In the living fish light is absent there. It may be a vestigial character. The author admits that in general "no attempt has been made to investigate the homologies of the parts described and some of the names of the bones used here may not be correct or may be out of date." The

illustrations, with their elaborate details and descriptions, serve to clear up any such slips.

WILLIAM BEEBE

THE REPTILES OF OHIO. *Second Edition Revised. By Roger Conant. University of Notre Dame Press, Notre Dame.* $3.50. 284 pp. 1951.

The first edition of this work appeared in 1938 (*Q.R.B.*, 14: 88. 1938), and in a relatively short time was soon out of print. The demand continued to be so heavy that a new edition was contemplated. The material has been expanded and revised by adding to the first reprinted edition a revisionary addendum. The latter includes all new records, additional life histories and ecological notes, recent taxonomic changes, a completely new set of maps, and a list of supplementary references. The index covers both parts. Those unfamiliar with the original work will probably do best by referring to the section in the rear of the volume first, since this constitutes the newest and most up-to-date information. Data are keyed by page, map, and plate numbers to the original. The wide acclaim of the original edition attested to its great value. The revision will be just as important, since it has been as carefully put together as its predecessor.

HENRI C. SEIBERT

THE PHEASANTS OF THE WORLD.

By Jean Delacour; illustrated by J. C. Harrison. Charles Scribner's Sons, New York and Country Life Limited, London. $35.00. 347 pp. + 32 pl. 1951.

This work, in which all the pheasants, from *Ithaginis* to *Afropavo*, are treated, stems from the pen of the best connoisseur of this bird group. For everyone who is concerned with pheasants, whether ornithologist, bird-lover, or breeder, it is quite indispensable, since Beebe's masterwork of 4 volumes, *Monograph of the Pheasants* (1918–1922), has long been out of print and on account of its high price available to but few. It must be added that knowledge of the pheasants has considerably increased in the past 30 years. That will be apparent to everyone who compares Beebe's distributional maps with those of Delacour.

For each genus the description begins with the distribution, the molting, a key to species, and information regarding the ecology and mode of life. There follows the discussion of the species and their geographical races, together with a short description of the plumage and the eggs, and precise data as to the geographic distribution, to which are added short sections on the general behavior, nutrition, calls, reproduction, and maintenance in captivity.

Delacour has studied the pheasants not only in museums, but has also seen many species in freedom during his own wide travels. Above all he has maintained for decades a very large number in his aviary in Clères in France, has bred them and thoroughly studied them. That gives to this work a great worth. Novel (but of doubtful value) is the fusion of the long-recognized genera, *Gennaeus*, *Hierophasis*, *Houppifer*, *Lophura*, *Diardigallus*, and *Lobiophasis* into a composite genus *Lophura*, in spite of all differences in morphological and also in biological respects.

The great importance of this work lies in those sections which have been written with the experience of the breeder and the attentive observer. Not only will the pheasant-keeper obtain important instruction from them, but also the student of behavior, to whom a wealth of material and stimulation for a comparative ethology of the fowls is offered here.

A special charm accrues to this work through its many plates, half of them in color, in which all species and a part of the subspecies have been pictured in characteristic poses, and indeed mostly from the life. Through these the artist, J. E. Harrison, has made for himself an enduring name as a bird painter.

E. STRESEMANN

BIRDS FROM LIBERIA *With a Discussion of Barriers between Upper and Lower Guinea Subspecies. Fieldiana: Zoology, Vol. 32, No. 9, Dec. 19, 1951.*

By Austin L. Rand. Chicago Natural History Museum, Chicago. $1.00 (paper). Pp. 561–653 + 1 pl. 1951.

In the lowland forest between Liberia and Nigeria there exists a gap formed by a savannah extending from Lagos to Accra. A study of the birds from this region reveals that while those of Liberia differ in some cases from those of Nigeria, there are too many resemblances to regard this grassland as an important barrier, so that Nigeria is here considered as belonging to the Upper Guinea lowland forest, together with Liberia and the Gold Coast. It is suggested that the Cameroon Highlands, with its former extensive mountain vegetation, acted as a more efficient barrier, one whose effects are still to be noted, and that that region may properly be regarded as the demarcation between the Upper and Lower Guinea lowland forest. This discussion is followed by a list of species, with commentary, of Liberian birds, based principally on a collection made in 1947 and 1948 by Harry Beatty.

HENRI C. SEIBERT

TAXONOMY OF THE KARROO AND RED-BACK LARKS OF WESTERN SOUTH AFRICA. *Vol. I, No. 11.*

By J. D. MacDonald. The British Museum (Natural History) London. 10s (paper). Pp. 319–350 + 3 pl.; text ill. 1953.

The British Museum South-West Africa Expedition (1949–1951) collected a sufficient number of larks of the genus *Certhilauda* to instigate this revision. Confusion has existed as to whether 2 species, a red and a grey, actually exist. The present information indicates that only 1 allopatric species, consisting of 7 races, 2 of which are newly described, is involved. Some races are gray, others red, and other populations may include both. Size does not differ. Variations in color and distribution of color are broadly correlated with the color of the soil and the pattern of the environment.

HENRI C. SEIBERT

ON THE SPECIES AND RACES OF THE YELLOW WAGTAILS FROM WESTERN EUROPE TO WESTERN NORTH AMERICA. VOL. I, NO. IX.

By C. H. B. Grant and C. W. Mackworth-Praed. The British Museum (Natural History,) London. 5s. (paper). Pp. 255–268 + 3 pl. 1952.

A critical study of the Yellow Wagtails convinces the writers that instead of 1 species, there are 7. This conclusion is derived from a study of 2594 specimens, with special emphasis on males collected on the breeding grounds. Maps indicate the breeding areas of the 7 species and their respective subspecies and what is known of their migrations.

HENRI C. SEIBERT

A GENERIC REVISION OF FLY-CATCHERS OF THE TRIBE MUSCICAPINI. *Bull. Amer. Mus. Nat. Hist., Vol. 100, Art. 4.*

By Charles Vaurie. American Museum of Natural History, New York. $1.00. Pp. 453–538; ill. 1953.

This study is concerned with 113 species of flycatchers of the tribe Muscicapini, within which 12 genera are recognized. Three of the latter, *Horizorhinus*, *Newtonia*, *Culicicapa*, are of uncertain affinity. The first is monotypic and may be a babbler, a warbler, or a thrush. The second may be distantly related to *Muscicapa*; the third has affinities with the Rhipidurini. The other genera consist of *Bradornis* (with the related *Melaenornis* and *Fraseria*) of Ethiopian distribution; the Indo-Malaysian *Rhinomyias*; and the wide-ranging *Muscicapa* (with related *Ficedula*, *Niltava*, *Microeca*, *Humblotia*). Since the shape and size of the bill, of the tarsi, the wings, and other morphological characters are considered adaptive, no single feature can be relied upon to indicate relationship. This is one reason why the taxonomic history of this group has been so varied and confused. Even feeding habits vary. All criteria, including pattern and coloration of plumage, must be weighed together. When this is done the above genera appear to be homogeneous except for *Ficedula*, within which several related subgroups can be distinguished. Many tables and drawings indicate that the validity of each character was thoroughly examined.

In such a difficult group of birds as this, there is bound to be a diversity of opinion and judgment and the many concepts as to what constitutes a genus in this tribe have enlarged the synonymy. A special appendix takes care of nomenclatural changes necessitated by this publication—other synonyms are listed in the body of the text. For each genus, incidentally, there is a list of species belonging to it and the range of each. The misprint "Musicapinae" occurs three consecutive times, an error worthy of note in these carefully edited bulletins.

HENRI C. SEIBERT

GEOGRAPHIC VARIATION IN THE RED-EYED TOWHEE OF THE EASTERN UNITED STATES. *Bull. Mus. comp. Zool., Vol. 107, No. 5.*

By J. C. Dickinson, Jr. Museum of Comparative Zoology, Harvard College, Cambridge. $1.30 (paper). Pp. 273–352; ill. 1952.

This critical study on the towhee (*Pipilo erythrophthalmus*) in the eastern United States, based on about 2300 specimens, clears up some relationships that were rather vague. Measurements of body parts, descriptions of the variation in the color of the plumage, and the color of the irides are given in detail. As a result of this analysis, 4 races are recognized: ***erythrophthalmus***, a large, small-billed, vividly colored, red-eyed form with a large amount of white on the rectrices, occurring in the northern segment of the species' range; ***alleni***, a small, medium-billed, pale-eyed race with very little white on the rectrices, in Florida; ***canaster***, a large, large-billed, pale race with *variable* eye color, medium amount of white in the rectrices, in the southwestern portion of the species' range; and ***rileyi***, a medium-sized, large-billed race, variable in eye color, and with little white on the rectrices, in the southeastern segment. Based on the assumption that these criteria are definitive, ***erythrophthalmus*** is shown to be mostly migratory; ***canaster***, migratory in part; ***rileyi*** spreads north, south, and west during the winter, especially south to Florida; and ***alleni*** is largely sedentary. Maps, keys, and good tabular data make the writer's conclusions convincing. Some suggestions are presented to explain the geographic variations.

HENRI C. SEIBERT

PAPERS OF THE MICHIGAN ACADEMY OF SCIENCE, ARTS, AND LETTERS. *Vol. XXXVII (1951), Part II: Zoology.*

Edited by Eugene S. McCartney and Frederick K. Sparrow. The University of Michigan Press, Ann Arbor. $1.00. vii + pp. 121–152 + 1 pl.; text ill. 1952.

A series of 4 papers, the first of which purports to show that the lows in pheasant populations in Michigan are not related to irruptions of fox populations. This report is brief, and the only results presented are a series of graphs depicting kill per 100 square miles. Although the graphs appear to reveal an inverse relationship, the writer assures us that statistically the high fox population of 1945 was not related to the low pheasant population in 1947. The second paper indicates that alkaline phosphatase activity occurs in the cytoplasm and nucleus of *Tetrahymena geleii* grown in an alkaline medium, but not when grown in an acid medium. The third paper provides further evidence to show the difficulties of random sampling. Eight persons were asked to select and measure 25 fish out of previously measured populations of several hundred. In all cases, in spite of the fact that the nature of the experiment was known to all, the selected samples averaged larger. In the final paper, *Oxygeneum pulverulentum* Forbes, a unique monotypic specimen of fish described from Illinois, is shown to be a hybrid *Campostoma anomalum pullum* X *Chrosomus erythrogaster.*

HENRI C. SEIBERT

ECONOMIC ZOOLOGY

ANIMALS AND MAN.

By G. S. Cansdale. Frederick A. Praeger, New York. $4.00. 200 pp. + 31 pl. 1953.

Briefly and neatly the Superintendent of the London Zoo has summarized man's effects on native animal populations through competition and conflict with them or indirectly through introducing foreign animals. Most of these accounts make sad reading and emphasize the need for saner applications of biological understanding. The bulk of the book, however, is on the economic side—Animals in the Service of Man as producers of food and clothing, beasts of burden, companions and assistants, or about animals included in man's sport or zoos, or vectors of disease to kinds in which man is more primarily interested. The writing is pleasantly informative and views the world from London. The photographs are of high quality, beautifully presented.

LORUS J. & MARGERY J. MILNE

ANIMAL GROWTH AND DEVELOPMENT

INITIATION À L'EMBRYOLOGIE GÉNÉRALE.

By Albert Dalcq. Masson & Cie, Paris. 920 fr. ii + 122 pp. + 1 pl.; text ill. 1952.

In the spring of 1951, Professor Dalcq presented 13 talks on embryology, broadcast from Paris by the *Université Radiophonique Internationale*, and it is on these lectures that the present volume is based. The volume is highly readable, in fact, particularly distinguished by the eloquence of its language; and the content, which points out the fundamental problems of embryology and discusses the modern attacks on them, is remarkably good. The book will make fine reading for those interested in embryology at the lay or professional level.

Some professional embryologists may wonder at the author's tendency to overemphasize his own work and that of his associates at the expense of other work equally important. In the field of teleostean development, for instance, which is the only one I am competent to judge myself, the interesting and able work of Tung and Devillers is cited, which is appropriate; but nowhere in the book is mentioned the name of Luther, who is the single worker in the field of teleostean embryology whose contributions are characterized by the depth and breadth of comparable studies on the amphibian. It is, however, not only with reference to his emphasis on the work of his own associates that one questions the choice of investigations discussed. It is a little shocking to find that a book written in otherwise good historical perspective never mentions the name of Harrison and attributes to Spemann and his group at Freiburg sole credit for the progress ("*essor*") of the experimental investigation of the amphibian egg.

No one book in embryology can, however, include all there is to be said—the author is at liberty to make his own selection of material to be included. Those who are interested in a well-developed and lucid exposition of Dalcq's interpretations of the experimental analysis of the early organization of the egg will be well rewarded by reading this volume.

JANE OPPENHEIMER

LILLIE'S DEVELOPMENT OF THE CHICK. *An Introduction to Embryology. Third Edition.*

Revised by Howard L. Hamilton; Advisory Editor, B. H. Willier. Henry Holt & Company, New York. $8.50. xv + 624 pp. + 24 pl.; text ill. 1952.

For the past several decades my own unpublished observations have indicated that the most battered and bethumbed single volume on any zoological reference shelf was sure to be a copy of Lillie's *Development of the Chick.* It is thus an occasion of singular rejoicing that this classic has been worthily republished,

brought up to date without omission of any of its valuable features, through the able efforts of H. L. Hamilton, the advice of Professor Willier, and the enlightened self-interest of Messrs. Holt and Company. In a day when the objective in college textbooks of biological subjects seems to be to see how much documentation can be omitted and how superficial and lecture-like the treatment can be made, it is a distinct pleasure to note that this edition makes a striking exception and that "Lillie" remains a standard reference work, simplified only where new research has warranted. The publisher has been more than generous in the matter of illustrations; most of the old ones are there, by some alchemy appearing sharper and fresher than in previous editions (it is necessary to admit one or two exceptions, out of nearly 300, that emerge a bit fuzzy); and the new photographs are beautiful indeed. The revised typography and format are kind to the eye and pleasant to the hand. Not the least of the additions is the frontispiece portrait of the original author.

From this photograph there look out, through the sober mask of the gentleman financier, the quick wise eyes of perhaps the least-known distinguished biologist of his generation. When one surveys the many printed tributes to Frank Lillie, on anniversaries, or after his death, one is impressed by the fact that the scientific community at large evidently thought of him as an administrator, as the prudent yet ambitious builder of his University's biological departments and of the Woods Hole Laboratory, as the conservative, able manipulator of money and power. Few people felt really in contact with the shy poetic mind behind the mask—his scientific intimates probably least of all, since they were in a position to recognize the depths of reserve, the complexity of thought behind the beautifully formulated musical statements which they caught and countered in discussion. But the Lillie tradition—the intuitive recognition of major problems from the material itself, the painstaking first-hand investigation, the intellectual and artistic exercise involved in attempts at complete understanding and formulation of the results—these have been communicated generously. Though Lillie never in any accepted sense of the word formed a "school" of embryology, it is gratifying to note that almost all of the new parts in the present revision of his only textbook have been necessitated, in part or wholly, by original researches performed by Lillie himself, or by his students or *their* students and students' students. Here we have a living monument indeed.

Hamilton has treated the text with utmost piety and has kept his new material in complete harmony with the original presentation. He has thus unostentatiously incorporated a large amount of new work in embryology and avian physiology. The staging of the embryo, with its splendid plates, the sections on gastrulation, on the nervous system, the urinogenital system, and the integument, are either new or largely so, and are excellent. Considerable simplification has been possible in the accounts of germ-cell formation and of early morphogenesis. These revisions seem to have resolved most if not all of the puzzling contradictions in the original text. It can be said—and this is the highest form of praise to an editor—that every alteration has improved the original and made it more useful to the serious modern student.

It is customary to include in a review a few suggested corrections or amendments. The following are accordingly offered as being of more than technical interest. In the interest of brevity, experimental results have occasionally been presented without the necessary qualifications. For example, in Chapter 4, where the subject of the morphogenesis of gastrulation is so neatly brought up to date, a careful rereading has failed to disclose that it is anywhere made clear that Spratt's carbon-marking experiments were performed on blastoderms that had been excised and explanted. Surely such a point should be given prominence. In general, one feels that the American (as against the European) experimental background has been a trifle overemphasized. This bit of mild nationalism may, however, serve partly to redress an unbalance recently created by one of our learned British colleagues. It is to hoped that such international competition in provinciality stops here.

Chapter 14 on the skeleton needs more extensive revision. The recapitulatory significance of the cartilage skeleton has for some time been questioned on the best of grounds, namely, the paleontological; and experimental work in teratogenesis and genetics have made contributions to the physiology of skeletal morphogenesis that should not be neglected even in a general embryological work.

Let us hope that Hamilton's efforts will bear as much fruit in their day as did the original, and that when the next revision of Lillie comes due some whole chapters on the mechanisms of the bewildering and beautiful phenomena here treated will of necessity be included.

D. RUDNICK

THE EPIGENETICS OF BIRDS.

By C. W. Waddington. Cambridge University Press, New York. $7.00. xvi + 272 pp.; ill. 1952.

Waddington's book expresses as its purpose to review the literature, mainly dating from the 1930's, which deals with the primary stages of morphogenesis in the chick. "Work on later stages is reviewed more cursorily." Of the 11 chapters in the monograph, 1 is devoted to technical methods, 5 directly or indirectly to problems of organization of the early blastoderm, 1 to biochemical mechanisms related to early develop-

ment. One chapter concerns itself with the development of organ-systems at later stages, one each with problems of plumage and of genetic mechanisms, and a final chapter considers "the development of the epigenetic organisation of the chick." The book contains an extensive bibliography and an adequate subject-index. Its usefulness might have been increased by the addition of an author's index or by the inclusion of references to authors in the general index.

It is of course not possible in a monograph of the present scope, or indeed of any, to cover all topics in the proportions that would be considered golden by every reader. To my own taste, the early chapters dealing with primary morphogenesis present too much detail, without covering generalization, to be readily read or digested by any except experts specializing in chick development, while the later chapters are so extremely cursory as perhaps to have been better omitted altogether.

So much, however, is a matter of personal opinion. A much more serious flaw of the book is its over-subjectiveness. One usual advantage of English scientific writing, as compared with American, is that it often tends to be expressed in more personal terms. A scientist is as much a subjective human being as other members of the species, his writing tends to be more interesting when couched subjectively; and when he reveals his feelings, as well as his observations, in his scientific writing, that may be so much the better in providing a basis by which the reader can judge the significance of his data and the validity of his interpretations. But Waddington has overstepped himself in this respect. "Muddled" and "confused" are poor words to use in the 20th century to describe the thought of those with whom one may indulge in honest disagreement as to interpretations of fact. And not only in words, but in many general attitudes, e.g., in generalizations concerning the thinking of "schools" which in fact have no existence, Waddington tends to weaken his own critical concepts. Now, when the world turns backwards in so many ways, it is especially discouraging to watch scientists return to the polemic style of criticism which we had hoped to leave behind with the 19th century. Subjectivity can be purchased at a far less heavy price.

A point about the polemic style of the 19 century was this: it was a "style," and perhaps some of the acerbity was assumed to be discounted by the reader. One wonders if this is true of some of what seems to be Waddington's harshness. This is worth some inquiry with respect to one of the fundamental points of his discussion, namely, the interpretation of the results of chorio-allantoic grafting. All modern investigators agree that this is an imperfect technique. It has some value, however, *within its limitations* (just as has the equally imperfect carbon-marking technique which Waddington records that he used before Spratt). But the investigators who have used the chorio-allantoic grafts would be the last to draw unwarranted conclusions from their data, and one wonders whether this is not tacitly admitted by Waddington by virtue of the inclusion in his book of a large inset folder reproducing the chorio-allantoic maps—an action which must greatly increase the cost of a book that is really extremely expensive for students to buy.

It is a very great advantage to have the material covered by Waddington discussed thoughtfully and exhaustively within the covers of a single monograph. It is to be hoped, however, that the book will soon see an early reedition which will allow the same material to be discussed just as critically but a little less polemically, so that those who are trying to interest the minds of a new generation in embryological problems can recommend the volume to them with fewer worries and reservations about the jeopardization of the scientific attitude than the present volume permits.

JANE OPPENHEIMER

POSTURAL DEVELOPMENT OF INFANT CHIMPANZEES. *A Comparative and Normative Study Based on the Gesell Behavior Examination.*

By Austin H. Riesen and Elaine F. Kinder, with a Foreword by Henry W. Nissen. Published for the Yerkes Laboratories of Primate Biology by the Yale University Press, New Haven. $5.00. xxii + 204 pp. + 5 pls.; text ill. 1952.

The age changes in man's postural behavior have been carefully recorded, but had to remain a descriptive catalogue with the nearly total lack of comparative data for other primates. This well-documented report on the physiologically conditioned postural development of chimpanzees opens new possibilities for explaining why we behave so little like human beings and so much like apes.

The basis for this study is composed of regularly repeated observations on 13 nursery-reared chimpanzees, and for comparison on some mother-reared ones as well as on some raised in human homes, all in their first year of life and all born in the Yerkes Laboratories of Primate Biology in Florida. The largest part of the book presents in detail for the chimpanzee the progressive changes in about 200 postural items of the behavior examination, developed for human infants by Gesell and Thompson. The postural patterns of the ape are much more similar to those of man at an early age than when developed to their full complexity, and hence the authors can readily compare their findings with corresponding ones for man in his infancy. The postural development of man and chimpanzee differs in some respects already at an early stage, in other respects not until late, but in a majority of the items

the developmental curves for these two primates show a close similarity of direction, combined with marked discrepancies in speed. On an average the ages of appearance of these developmental items are one-third smaller in chimpanzee infants than in human infants, but in regard to some few single items the critical ages differ far more and can even occur much earlier in man than in the ape. Among the chimpanzees differences in rate of postural development are significantly correlated with the known length of gestation as well as with the ages of dental eruption, but seem to be little influenced by environmental variations. The data of this study show no differences according to sex, a result which is not surprising, since chimpanzees are noted for their small secondary sex differentiation. In general, it is found that the age changes in posture correspond in ape and man more closely for head and trunk than for the extremities. This may be connected with the fact that many of the postural items appertaining to the limbs are not really "developmental," but represent unilateral activities or limb flexions which are persistent in chimpanzees. A conclusion of great interest is the authors' statement: "The data provide ample confirmation of the impression that the chimpanzee is an 'upright' animal, and that he develops toward vertical posture in the first year of life following a course very similar to that of the human infant."

The good photographs of typical postures of chimpanzee babies at different stages of their first year's development add much to the value of this pioneering study, which also contains a welcome bibliography of 93 references.

A. H. SCHULTZ

HIPPOCRATES ON INTERCOURSE AND PREGNANCY. *An English Translation of "On Semen" and "On the Development of the Child".*

Translated by Tage U. H. Ellinger with an introduction by Alan F. Guttmacher. Henry Schuman, New York. $2.50. 128 pp. 1952.

This is the first English translation of *On Semen* and *On the Development of the Child*, "the very fountainhead of embryologic theory and observation," according to the introduction by Alan F. Guttmacher. In addition, the text contains considerable of Darwin's theory of Pangenesis.

J. F. CASE

ANIMAL MORPHOLOGY

THE VERTEBRATES. *A Work Book in Animal Biology. By F. Whitwam Jones. J. M. Dent & Sons, London.* 1s. 9d. (paper). 48 pp.; ill. 1950.

A small workbook suitable only for students in the secondary school level.

RUTH NEFF

ATLAS AND LABORATORY MANUAL FOR THE DISSECTION OF THE SHARK.

By William Henry Atwood. Burgess Publishing Company, Minneapolis. $2.00 (paper). iv + 40 pp. + 18 pl.; text ill. 1951.

This manual is allegedly a new guide for the dissection of the dogfish, but it is difficult to see in what manner it improves upon a number of mediocre atlases and manuals already available. The directions for dissection are neither explicit nor complete, and there are numerous serious errors in content. An example of the extremely careless diction is the following: "... the *opthalmicus superficialis*, ... *maxillaris*, ... *mandibularis*, ... and the *opthalmicus profundus*, which we have seen, are the four branches of the trigeminal nerve as it is found in sharks, where it is a mixed nerve carrying fibers of the fifth and seventh nerves." There are 13 figures and 19 plates, all of which are conventional in planning, execution, and reproduction. There is no consistent effort to relate structure to function, although a few gestures toward that end have been made.

RUTH NEFF

DISSECTION GUIDES. *II. The Dogfish.*

By H. G. Q. Rowett. John Murray, London. 4s. (paper). 62 pp.; ill. 1950.

This small manual is a series of drawings designed to aid dissection with a minimum of written instruction. No attempt is made to describe the form or relation of the parts. The drawings are not very clear, and do not demonstrate essential relationships at all, and unfortunately incorporate errors which would prove more confusing than enlightening to the student.

RUTH NEFF

PRACTICAL ANATOMY OF THE DOGFISH, NECTURUS, AND CAT. *2nd Edition.*

By George C. Kent, Jr. Wm. C. Brown Company, Dubuque, Iowa. $2.50 (paper). 82 pp.; ill. 1950.

Kent's manual is a laboratory guide for a standard course in comparative anatomy, and includes directions for the forms listed in the title, and a short section on the comparative osteology of skulls. A limited number of accurate and clearly labeled diagrams are scattered through the text, but this guide is in no sense an atlas. The plan for dissection is by the organ system plan

for the dogfish and *Necturus*, but a preponderantly regional system is adopted for the cat. The manual includes several useful tabulations, and except for the fact that some of the major principles are not strongly emphasized, it should prove satisfactory as a laboratory guide.

RUTH NEFF

FUNCTIONAL ANATOMY OF THE MAMMAL. *Second Edition. A Guide to the Dissection of the Cat and an Introduction to the Structural and Functional Relationship between the Cat and Man.*

By W. James Leach. McGraw-Hill Book Company, New York, Toronto, and London. $4.50. x + 276 pp.; ill. 1952.

This textbook is intended primarily for use in a vertebrate anatomy course for liberal arts students, or for students in special curricula, such as nursing and physical education, and as such it is excellent. The description of the anatomy of the cat, the representative mammal chosen for intensive study, is exceptionally thorough for a short book, and the illustrations are abundant and lucid. Although the laboratory animal is the cat, the text is also suitable for elementary courses in human anatomy, since the homologies between cat and man are emphasized, and the distinctive features of each form are interpreted in terms of their respective postures. Following orienting chapters which survey landmarks of superficial anatomy and the major divisions of internal anatomy, each organ system is discussed in a separate chapter, and is accompanied by a consideration of the elementary physiology of each system.

The text is accompanied by well organized tabulations of anatomical detail with respect to major blood vessels, cranial nerves and spinal nerves, etc. Lists of anatomical terms are interspersed in the text, with brief definitions for each. Each chapter closes with a group of well-written topics for discussion. In addition to its merit as a textbook for the special groups for which it was designed, this book should also prove useful to the student of general zoology and comparative anatomy as a supplementary reference.

RUTH NEFF

ANATOMY OF THE CHORDATES.

By Charles K. Weichert. McGraw-Hill Book Company, New York, Toronto, and London. $8.00. viii + 921 pp.; text ill. 1951.

Weichert has written a fluent and well-organized textbook of chordate anatomy, liberally seasoned with natural history. The book is organized into 3 major divisions.

Part I is, for the student, both a review of familiar facts and a stimulating introduction to an intensive study of the chordates. The chapter on classification and natural history deals chiefly with living chordates, with only cursory references to extinct ancestral types. In this division, there is also a brief account of the early embryology of vertebrates, and a concise summary of vertebrate histology.

In Part II, the major organ systems of the vertebrates are considered. Their embryological development is described, and then each organ system is discussed thoroughly from the comparative point of view. One of the most pleasing features is the effort to integrate variations in structure with functional patterns. Sufficient elementary physiology is presented along with the account of structural differences to aid the student in interpreting morphological adaptations and specializations. However, the chapters on the organ systems are written in a less stimulating style than the introductory chapter on natural history, and are conservative in their treatment of relatively recent concepts, e.g., that of the opistonephros. The illustrations in this section are conventional, many of them having been adapted from the classical comparative anatomy sources. Finally, in these chapters it is the variations in the organ systems which receive emphasis rather than the underlying similarities, a stress which may obscure phylogenetic relationships. This minimal treatment of evolutionary patterns and trends is, in my opinion, the principal weakness of this text.

Part III is entitled Representative Animals, and contains concise and accurate descriptions of the morphology of the lamprey, dogfish, mudpuppy, and cat. This section, containing numerous drawings and diagrams, should be a valuable aid to the student in the laboratory.

RUTH NEFF

A LABORATORY GUIDE TO THE ANATOMY OF THE RABBIT.

By E. Horne Craigie. The Blakiston Company, New York, Toronto, and Philadelphia. $3.25. viii + 113 pp.; text ill. 1951.

This dissection guide is intentionally limited in scope, and is designed for a brief course in mammalian anatomy, using the rabbit as subject. The dissection directions follow the regional method of dissection, but the author has attempted to arrange material in such a way as to emphasize the totality of organ systems as much as possible. The limited scope of the text means that only major arteries, veins, and nerves can be considered, but those which are described are discussed thoroughly and in relation to their functions.

RUTH NEFF

FUNCTIONAL MAMMALIAN ANATOMY (*with Special Reference to the Cat*).

By William T. Taylor and Richard J. Weber; illustrated by Thomas Courtney Lee. D. Van Nostrand Company, New York, Toronto, and London. $7.25. xii + 575 pp.; ill. 1951.

This textbook is an accurate and highly detailed description of the anatomy of the cat, with a discussion of function introducing or closing the chapter on each organ system in order to orient the student. Yet even though the authors enumerate some of the functions of the organs to be dissected, they seem to make little effort to integrate structure with function. There is a section defining terms in each chapter, and abundant and lavish color plates accompany the text. The last chapter is a manual for the regional dissection of the cat in the laboratory, with frequent cross references to other parts of the book and to the plates. The great mass of detail in the text makes it extremely valuable as a reference book for the student of comparative anatomy, but somewhat unwieldy for the short courses offered in mammalian anatomy for nursing, public health, and physical education students.

RUTH NEFF

MANUAL FOR DISSECTION OF THE CAT.

By Chester B. Dugdale. Burgess Publishing Company, Minneapolis. $1.65 (paper). iii + 43 pp.; ill. 1949.

Each of the fourteen exercises in this manual begins with a check-list of all structures to be identified by the student in that exercise. Each check-list is organized in outline form, so as to emphasize the anatomical classification of these structures. With the exception of the check-lists, and very brief general instructions, there are no specific written directions for dissection. The illustrations are abundant only in the exercises on skeleton and muscles (the skeleton described being that of man). There are 2 useful diagrams showing the essential relationships of major arteries and veins.

This manual contains no material on the functions of the parts listed, e.g., on muscle actions and attachments. Any instructor choosing it would find it difficult to use without extensive supplementation from outside sources.

RUTH NEFF

ANGEWANDTE UND TOPOGRAPHISCHE ANATOMIE. *Ein Lehrbuch für Studierende und Ärzte.*

By Gian Töndury. Georg Thieme Verlag, Stuttgart; [Grune & Stratton, New York]. DM 70.00; [$16.65]. 416 pp. + 133 pl.; text ill. 1951.

This is a new applied and topographical anatomy book emanating from the University of Zürich. The illustrations are almost entirely new and are admirably presented, especially those of R. Winkler. Failure to number figures and plates consecutively is not unusual in European publications, but is rather disconcerting. Otherwise the format is quite conventional, and the book as a whole is decidedly above average in its class. It is recommended to anyone who has need for a reliable, authoritative German anatomy text in one volume.

F. N. LOW

LABORATORY MANUAL IN ANATOMY AND PHYSIOLOGY. *Second Edition.*

By Elizabeth Carpenter Earle. F. A. Davis Company, Philadelphia. $2.00 (paper). viii + 186 pp.; ill. 1950.

An elementary teaching manual designed for students of nursing.

THE FOOT. *Fourth Edition.*

By Norman C. Lake. Baillière, Tindall, and Cox, London; The Williams & Wilkins Company, Baltimore. $5.50. v + 466 pp.; ill. 1952.

The present edition of this book contains 30 new illustrations. Its purpose, as expressed in the first edition (1935), is to deal with those conditions likely to be encountered by the general practitioner and surgeon. Introductory chapters on the evolution, development, and anatomy of the foot, and even the history of footwear, are followed by primarily clinical considerations of afflictions of the foot. The illustrations are adequate and the style easy to read. The subject is covered competently, but it is not a reference work. There are more scholarly tomes on the wisdom of the foot (Morton, 1935; 1952) but this one is so written as to be useful, as the preface states, "... to the Chiropodist, Physiotherapist and other Medical Auxilliaries, even though its scope is beyond the requirements of those undertaking these ancillary services."

F. N. LOW

A TEXTBOOK OF HISTOLOGY. *Ninth Edition.*

By Harvey Ernest Jordan. Appleton-Century-Crofts, New York. $9.00. vii + 687 pp.; ill. 1952.

This textbook is recognized in its field as substantial and reliable. The present edition conforms to previous

ones without change in format. The work of the past 5 years, notably that in electron microscopy, has been incorporated into the text, with new illustrations; but descriptions and interpretations follow patterns that have remained essentially unchanged through many editions. This criticism is not intended to disparage its approach, but rather to contrast it with more dynamic interpretations of histology that have developed largely in response to the last three decades of research. Those who are familiar with this textbook will find the present edition a satisfying continuation of its tradition.

F. N. Low

BIOLOGY OF THE TESTES. *Ann. N. Y. Acad. Sci., Vol. 55, Art. 4.*

Edited by Roy Waldo Miner. New York Academy of Sciences, New York. $3.75 (paper). Pp. 543–742; ill. 1952.

The 17 papers in this article center about fields of investigation in male reproduction, with particular reference to their bearing on infertility. An introductory paper summarizes the 4 following groups of 4 papers each.

The first group deals with the epididymis and transport of sperm cells. Proper structural maturation of sperm does not guarantee fertility, since enzymatic changes must follow. The second group of papers deals with the possibility that physiological patterns up to puberty may affect spermatogenic capacity. Related cytochemical techniques are discussed. The third group deals mainly with testicular biopsy as a diagnostic procedure in infertility; and the last with steroids and spermatogenesis. This is a book by specialists for specialists. It should be very useful in its field.

F. N. Low

THE DISTRIBUTION OF SUBCUTANEOUS FAT IN CHILDHOOD AND ADOLESCENCE. *Monogr. Soc. Res. in Child Development, Vol. XV, Ser. No. 50, No. 2. 1950. From The Fels Research Inst. for the Study of Human Development, Series No. 1.*

By Earle L. Reynolds. Child Development Publications, Evanston. $2.25. xviii + 189 pp.; ill. 1951.

This monograph reports an extended study of soft tissues in children during growth. A group of 300 white children was x-rayed and measured annually for a period of 1 to 11 years at ages from 6.5 to 17.5 years. The results are presented in 61 tables and 102 figures, in sufficient detail to permit critical analysis of both method and results. This volume should be an acceptable addition to the library of those interested in child growth.

F. N. Low

FUNDAMENTALS OF THE HISTOLOGY OF DOMESTIC ANIMALS.

By Alfred Trautmann and Josef Fiebiger; translated and revised from the Eighth and Ninth German Editions, 1949, by Robert E. Habel and Ernst L. Biberstein. Comstock Publishing Associates, a division of Cornell University Press, Ithaca. $8.75. xiii + 426 pp.; ill. 1952.

This translation was undertaken because ordinary textbooks of histology are not adequate for veterinary study of the digestive, genital, and integumentary systems. The general format follows that of classic histology texts. The style is simple and the subject material quite elementary. Most of the 450 figures are halftones but there are numerous color plates and drawings. Illustrations are usually identified by species, and many structural variations little known to the average histologist are presented. The refinements of histological distinctions among commonly used laboratory animals are not discussed. Reference is limited to a few key volumes. This textbook is accurate, easy to read, and apparently well suited to the needs of veterinary students. Its usefulness elsewhere is limited to the unusual material it contains.

F. N. Low

ATLAS DER NORMALEN HISTOLOGIE UND MIKROSKOPISCHEN ANATOMIE DES MENSCHEN.

By E. Von Herrath and S. Abramow. Georg Thieme Verlag, Stuttgart; [Grune & Stratton, New York]. DM. 48.00. 139 pp.; ill. 1950.

This neat volume is an interesting tribute to postwar recovery in Germany. It was prepared cooperatively by the free University of Berlin and the Leitz Optical Works of Wetzlar, being dedicated to Doctor Ernst Leitz on the hundredth anniversary of this company.

A 10-page explanation describes the photomicrographic technique and the salient characteristics of the illustrations. There are 398 figures, almost all in color, of fundamental tissues and organs. Each micrograph is labeled by black guide lines with printed initials which are identified in the accompanying legend. The legend also describes the micrographic technique, listing lens type, magnification, exposure time, and illumination. Most of the tissue is human. Fixation and staining are excellent. A wide variety of stains was used, some of them quite unusual, but basic structure is well pictured. The color micrographs are nearly flawless.

The technical skill presented here will not be easy to duplicate. The volume could serve as a model for those interested in color micrography. Although of limited scope, it should be very useful for demonstration purposes in histology courses at any level of instruction. Doctors von Herrath and Abramow deserve great praise for their achievement.

F. N. Low

CONNECTIVE TISSUES. *Transactions of the Third Conference, February 14–15, 1952, New York.*
Edited by Charles Ragan. Josiah Macy, Jr. Foundation, New York. $3.50. 166 pp.; ill. 1952.

The report under consideration here merits great praise, as did the previous one (*Q.R.B.*, 28: 86. 1953). Precise presentations and informal discussions cover the following topics: Connective Tissue Staining, (R. D. Lillie); The Fine Structure of Connective Tissues, (R. W. G. Wyckoff); The Nature of Reticulin, (A. H. T. Robb-Smith); and Hypersensitivity and the Hyperadrenal State, (E. E. Fischel). Each of these articles is of considerable interest both for its content and its relationship to our knowledge of connective tissues in general.

Studies in electron microscopy are gradually making ultrastructure in connective tissue visible in such a way as to require collation with the classic patterns of light microscopy. The need for continued investigation of connective tissue ultrastructure, both fibrous and cellular, is well emphasized in these discussions.

In spite of the splendid quality of this report, one is obliged to be critical of certain technical features. The binding, for example, is rather poor; pages tend to come loose after even moderate handling. Without the jacket the book is quite unattractive. The color plates facing pages 28 and 29 are good, but the legends are reversed. The quality of the contents merits some improvement in physical appearance. Otherwise recommendation is without reservation.

F. N. Low

ANIMAL PHYSIOLOGY

ANNUAL REVIEW OF PHYSIOLOGY. *Volume 14.*
Edited by Victor E. Hall; Associate Editors, Jefferson M. Crismon and Arthur C. Giese. Annual Reviews, Stanford. $6.00. x 589 pp. + 1 pl. 1952.

The 1952 *Annual Review* has adequate coverage of the foreign literature in most but not all of the topics treated. Three of the reviews are by workers in European laboratories. Physiologists continue to be in debt to those who do the great amount of work required to produce these reviews.

The topics covered are: The Organization of Science (R. W. Gerard); Physical Properties of Protoplasm (D. F. Waugh); Growth (L. J. Wells); Physiology of the Connective Tissue (C. Ragan); Physiological Effects of Heat and Cold (S. Robinson); Energy Metabolism of Biosynthesis at the Cellular Level (S. Spiegelman and M. Sussman); Water Metabolism (J. R. Robinson and R. A. McCance); Respiratory System (J. L. Whittenberger and J. V. Maloney, Jr.); Comparative Physiology of Invertebrate Muscle (C. A. G. Wiersma); Physiology of the Digestive System (C. M. Wilhelmj); Fundamentals of Blood Clotting (J. E. Flynn and R. W. Coon); Blood Gas Transport (E. H. Wood); Peripheral Circulation (J. R. Pappenheimer); Heart (G. Biörck); Lymphatic System, (R. L. Webb); Kidney (A. C. Corcoran, H. Dustan, and G. Masson); Excitation, Conduction and Synaptic Transmission in the Nervous System (C. McC. Brooks and M. G. F. Fuortes); Somatic Function of the Central Nervous System (M. Hines); Visceral Functions of the Nervous System (A. Kuntz); Hearing (B. E. Gernandt); Pituitary-Adrenal System (J. W. Conn and S. S. Fajans) Thyroid Gland (A. Albert); Reproduction (C. G. Hartman); and Physiology of the Skin (E. M. Farber and W. C. Lobitz, Jr.).

PAUL CRANEFIELD

AIDS TO PHYSIOLOGY. *Fourth Edition.*
By Henry Dryerre. Baillière, Tindall & Cox, London; [Williams & Wilkins Company, Baltimore]. $2.25. viii + 327 pp.; ill. 1951.

As the title implies, this little book may help as a refresher course for those who have previously had physiology or related subjects and are preparing for examinations.

METABOLIC INTERRELATIONS. *Transactions of the Third Conference, January 8–9, 1951, New York.*
Edited by Edward C. Reifenstein, Jr.; Clydelle Brownfield, Editorial Assistant. Josiah Macy, Jr. Foundation, New York. $4.00. 294 pp.; ill. 1951.

This is a report of a conference the purpose of which was to bring together some 25 investigators from various related disciplines, and who are interested in biochemical, histological, embryological, and biophysical aspects of bone. The object was to break down the "isolation" of these several branches of sciences brought about by the difficulty in communication, particularly because of the accelerating rate at which new knowledge is accumulating today. In this book we have the discussion of the following 14 subjects: Resorption of Bone; Some Histochemical Observations on the Development

of Membrane Bone; Studies on the Effect of Alteration in the Concentration of Calcium in Circulating Fluids on the Mobilization of Calcium; Osteogenesis; Osteogenetic Potency and Osteogenetic Inductor Substances of Periosteum, Bone Marrow, Bone Grafts, Fracture Callus, and Hyaline Cartilage Transferred to the Anterior Chamber of the Eye; A Further Consideration of the Effects of Beryllium Salts on In Vitro Calcification of Cartilage; Inhibition of Endochondral Calcification In Vitro by Beryllium and L-Histidine; Summary on Osteogenesis; Effects of Pituitary Factors and Thyroxin on Skeletal Morphogenesis in the Rat; The Nature of Bone and Phosphate Rock; Some Considerations of the Solubility of Calcium Phosphate; Observations on the Role of Phosphatase; The Effect of Age and Low Phosphorus Rickets on Calcification and the Deposition of Certain Radioactive Metals in Bone; Retention and Turnover of Radiocalcium by the Skeleton of Large Rats; and the Electron Micrography of Bone.

DAVID B. TYLER

THE INFLUENCE OF HORMONES ON ENZYMES. *Ann. N. Y. Acad. Sci., Vol. 54, Art. 4. December 28, 1951. Edited by Roy Waldo Miner, with R. I. Dorfmann, and E. D. Goldsmith. The New York Academy of Sciences, New York.* $3.50 (paper). Pp. 531–728; ill. 1951.

This is an impressive series of 16 articles in which "the oldest of the arts and the newest of the professions" makes an approach to the question of the mechanisms of hormone action. The majority of the papers deal with steroids, but insulin, pituitary hormones, and thyroxin are also given some attention. Although one must withhold judgment as to the significance of observations on homogenates for processes in vivo, these studies represent a development which no endocrinologist can ignore. The editors should be congratulated for having assembled a valuable group of contributions which are presented compactly but without undue compression.

EVELYN HOWARD

METABOLIC METHODS. *Clinical Procedures in the Study of Metabolic Functions.*

By Frank Consolazio, Robert E. Johnson, and Evelyn Marek. The C. V. Mosby Company, St. Louis. $6.75. 471 pp.; ill. 1951.

This is a useful manual of methods for studies on mammalian metabolism, particularly on human beings. The techniques have been carefully worked over to make them suitable for routine use where speed, simplicity, and reproducibility are desired. There is an excellent section dealing with procedures suitable for studies in the field. The book is well indexed and can be recommended for those who desire a manual in which a uniform presentation of methods is desirable.

DAVID B. TYLER

ADRENAL CORTEX. *Transactions of the Third Conference, November 15–16, 1951, New York.*

Edited by Elaine P. Ralli. Sponsored by Josiah Macy, Jr. Foundation, New York. $3.25. 204 pp.; ill. 1952.

There certainly is need for a better synthesis of the data which pour out of our laboratories. The Macy Foundation is conducting an interesting experiment in synthesis by group interchange. Although these Macy conferences are necessarily uneven in quality, and often characterized by a considerable amount of discussion which seems to plateau along a good deal of "we haven't tried that," they are nevertheless frequently a fairly stimulating brew. This one seems particularly successful from the viewpoint of one who is perennially preoccupied with problems of the adrenal gland.

EVELYN HOWARD

PHYSIOLOGY OF SHOCK.

By Carl J. Wiggers. Commonwealth Fund, New York. $5.00. xx + 459 pp.; ill. 1950.

In this monograph the emphasis is principally on the work of Wiggers and his coworkers at Western Reserve University. Nevertheless, the author has also succeeded in critically evaluating well over a thousand papers dealing with some of the following aspects of the subject: Clinical Shock, Criteria and Production of Experimental Shock, Experimental Hemorrhagic Shock, Methods for Studying the Hemodynamics of Shock, Mechanisms of Peripheral Circulatory Failure, The Heart in Shock, Respiratory, Oxidative and Metabolic Functions and Disturbances in Shock, Toxemic and Neurogenic Factors, and the Involvement of Special Organs in Shock. This book undoubtedly will be of great interest to the specialist in this field.

DAVID B. TYLER

SHOCK AND CIRCULATORY HOMEOSTASIS. *Transactions of the First Conference October 22–23, 1951, New York.*

Edited by Harold D. Green. Josiah Macy, Jr. Foundation, New York. $3.50. 245 pp.; ill. 1952.

This conference purposes to further "knowledge about

shock and circulatory homeostasis" and to promote "meaningful communication between the scientific disciplines" engaged in the study of shock. A series of 5 papers is presented together with generally pertinent discussion by a group of 23 conference participants. An excellent summary of peripheral vascular mechanisms as elucidated by omental and mesenteric studies is given by Benjamin Zweifach. Jacob Fine gives an absorbing account of the infectious element in shock; and Sharpey Schafer presents interesting data on hypotension after anesthesia in man. Less illuminating sections are those of John Remington and Francis Moore on the nervous system in shock and the therapeutic implications of shock.

In general, the conference purposes are realized and the book is exceptionally stimulating because of its diversity of material and viewpoints. There is much fine detail about dibenamine, VEM and VDM, radiation shock, hypotensive anesthesia, intraarterial transfusion, and so forth. Shock itself is not defined, but certain important concepts become quite clear: (1) Constant in shock is a prolonged deficiency of peripheral flow, and this is generally the aftermath of protracted hypotension. (2) Shock is producible in multiple ways (e.g., hemorrhage to constant volume or constant pressure, diverse trauma, intraperitoneal glucose, etc.), but they are not strictly comparable. (3) Numerous factors (e.g., renal, adrenal, sympathetic, etc.) modify shock and complicate study of the basic mechanism. (4) Irreversible shock is not an absolute term. Its study is concerned with how animals and human beings die.

Divergent points of view are generally brought closer together, but frequently are not reconciled or even clearly defined. Aside from this and limitations of space, the conference shows few deficiencies and can be recommended for study at all medical levels.

E. CONVERSE PEIRCE, 2ND

THE METABOLIC RESPONSE TO SURGERY.
By Francis D. Moore and Margaret R. Ball; metabolic diagrams by Mildred B. Codding. Charles C Thomas, Springfield, Ill. $7.50. xv + 156 pp.; ill. 1952.

When a primitive mammal lay injured and licking his wounds he was too weak to forage for food and water. His descendants inherited the stress or alarm reaction which mobilized body protein and electrolyte to the repair process. Now that the surgeon with his flask of amino acids and electrolytes can administer to the needs of the injured patient, the alarm reaction becomes somewhat of a nuisance. Early after operation, body protein is catabolized and its nitrogen cast into the urine. Early attempts to restore this by intravenous amino acid are of no avail. Potassium is rejected; sodium retained. The intensity of this response depends upon the degree of depletion previously suffered by the patient. The wise surgeon will attenuate the venous drip so that it complements losses and does not aggravate excesses. The authors have set up the detailed balance charts for nitrogen, potassium, and sodium that are required to accomplish this.

An early chapter describes the metabolic response as a function of the extent of damage imposed by the surgery. The next section attempts to explain why the response occurs, whether it is merely a reaction to starvation and bed rest, or whether it is entirely an adrenal cortical response to injury. A considerable nod is given to the latter. There follows a consideration of the influence of the presurgical condition of the patient, for instance, gross bodily depletion, upon his response. A rather special section on hypokaliemia suffers only from the fact that much has already happened in this field since the book was written. Actually the book anticipates much of this development, for this is somewhat the special domain of the authors. While they give a small number of determinations of total exchangeable mass of body potassium by isotope (K^{42}) dilution they wisely do not dwell upon this subject, pending further proof of just how dependable is the technique. Following a section on burns, there is a special section describing efforts to modify the response.

Instead of an alphabetical index the book has a very complete and full summary, called Facts and Corollaries. In fact, the entire organization is so clear-cut that an index would be superfluous. A final appendix lists the mineral and nitrogen content of the several post-surgical diets usually recommended on Dr. Moore's wards and of the several intravenous solutions commonly used.

The book is cast in a lively conversational tone. Some expressions might hurt the ear of a purist, but to me the vivacity attained is worth the risk. The volume is a monograph in the series, American Lectures in Surgery, edited by Michael E. DeBakey and R. Glen Spurling. The material began as a lecture series for Harvard medical students and was later delivered as part of the Basic Science Course, United States Army, Walter Reed Hospital. Its perusal will enliven the material taught by the physiologist or biochemist and add dignity to that taught by the surgeon or internist. The book carries all the acumen already seen in Moore's original articles in scientific journals. The extensive charts showing nitrogen, potassium, and sodium balance are alone worth the cost of the book.

WALTER S. WILDE

CURARE AND ANTI-CURARE AGENTS. *Ann. N. Y. Acad. Sci., Vol. 54, Art. 3. October 30, 1951.*
Edited by Roy Waldo Miner; K. R. Unna, Conference

Chairman, and 30 Contributions. New York Academy of Sciences, New York. $4.00 (paper). Pp. 297–530; ill. 1951.

A brief history of the interesting drug curare is given in several of the introductory chapters. Two papers are concerned with the pharmacologic characteristics of the active principles of the various types of curare. The remaining chapters deal with the synthesis of curare-like compounds, their pharmacology, and the clinical evaluation of curarizing drugs. Extensive bibliographies are given with each chapter. This is the most recent comprehensive review of the curare and anti-curare agents, and the volume should serve as a helpful, authoritative guide in this field.

C. JELLETT CARR

MODERN ELECTROCARDIOGRAPHY. *Volume 1. The P-Q-R-S-T-U Complex.*

By Eugene Lepeschkin; Foreword by Frank N. Wilson. The Williams & Wilkins Company, Baltimore. $12.00. xiv + 598 pp.; ill. 1951.

Primarily intended for the clinical electrocardiographer, this book offers definite advantages to the biologist who needs information on the ECG. There are 2 chapters on general techniques which include much material pertinent to animal work. There are chapters on the ECG of animals, and the effects of physical agents, drugs, hormones, and vitamins on the ECG. These chapters are chiefly concerned with the vertebrate ECG. The outstanding merit of the book is a large and very useful bibliography, which must include virtually all major and minor papers in U. S. and foreign journals from 1934 to 1950. This bibliography lists over 10,000 papers. References to papers prior to 1934 are to be found in an earlier book by the author. Although I do not agree with all the electrophysiological interpretations made in this book, it is to be noted that all points of view are carefully represented in the bibliography. About half of the book, not specifically discussed above, is concerned with the interpretation of the pathological ECG of human beings, and has the same bibliographical richness. The book should be very useful in the library of any biology department where work depending on ECG measurements is being done, and should be of interest to any worker in the field of cardiac electrophysiology.

PAUL CRANEFIELD

RENAL FUNCTION. *Transactions of the Second Conference, October 19–20, 1950, New York.*

Edited by Stanley E. Bradley. Josiah Macy, Jr. Foundation, New York. $3.00. 178 pp.; ill. 1951.

RENAL FUNCTION. *Transactions of the Third Conference, October 18–19, 1951, New York.*

Edited by Stanley E. Bradley. Josiah Macy, Jr. Foundation New York. $3.50. 210 pp.; ill. 1952.

These conferences "are not designed to present neat solutions to tidy problems, but to elicit provocative discussion of the difficulties which are being encountered in research." The second conference had as its topic the hormonal control of renal function, and the third conference, physical factors in urine formation. The reports of these two conferences will probably be of interest chiefly to persons enjoying similar difficulties.

EVELYN HOWARD

ANIMAL NUTRITION

THE GEOGRAPHY OF HUNGER.

By Josué de Castro; foreword by Lord Boyd-Orr. Little, Brown & Company, Boston. $4.50. xiv + 337 pp. 1952.

The author who is both a physiologist and a high official in the Food and Agriculture Organization of the United Nations, is well qualified to speak of hunger—this is his field of research as a scientist, and his area of interest as an executive. The result is a book that every thinking person should read even though the contents are grim indeed. The urgency for action, however, is grimmer. One must first define the type of hunger of which de Castro speaks. It includes not only the hunger that comes from sheer starvation, as seen in India and in China; it also includes that hidden hunger which results from a lack of proper vitamins and minerals, and that leads to a lassitude and an inefficiency which can seriously interfere with the economics of a nation or a region. It is this last type of hunger that spans the globe, and that may be found in regions where food, if measured by bulk alone, is plentiful and cheap. This has been particularly true of our own United States, where in the South the lack of certain vitamins has led to a widespread distribution of deficiency diseases. Pellagra, due to a lack of nicotinic acid, is one such manifestation, and even in 1938 it was estimated that in the Southern United States there were over 100,000 individuals suffering from this disease.

As Chairman of the Executive Council of the FAO, de Castro has also been concerned with the curing of the world's hunger ailments, for he believes, as do many others, that many of the disastrous wars have had their origin in the hungry bellies of downtrodden peoples. Two courses are open for the alleviation of these ills; reduction of population through birth control, and increased production from the arable lands of the world, coupled with a more equitable distribution of foods. Regarding increased production the author has but little to say, except that the efficiency of marginal

peoples and lands can be increased by better and more foods for the people and more scientific treatment of the land. In handling the question of overpopulation, de Castro arrives at a unique answer to the question of the differential fecundity of nations. In essence, it may be summed up as follows: the reproductive capacity of a population decreases as the protein content of its diet increases. Stated otherwise, starving peoples have a higher reproductive capacity than well-fed peoples, and coupled with this phenomenon is the further fact that persistent malnutrition increases sexual desires. Thus, the overpopulation of such a nation as India would be interpreted as due to increased fertility arising out of a low-protein diet and reinforced by increased sexual stimulation, rather than to a lack of knowledge of the methods of birth control. Even if de Castro is correct in his correlation of diet and fertility, the baldness of the statement glosses over much that can contribute to both fertility and to infant mortality. A high-protein diet would of course increase the resistance of infants to disease and thus offset to some extent a lowered birth rate. Furthermore, an evaluation of the social, economic, and educational pressures on the birth rate (which in most studies seem to show an inverse relationship) must be made and balanced against any increase in fertility brought on by diet before the entire picture of population control can be had.

Regardless of whether one accepts de Castro's hypothesis or not, the book is well worth the reading. It has brought into sharp focus one of the major ills of the world, if one, unfortunately, which is likely to remain so long as men are men and profits must be made.

C. P. SWANSON

BIOPHYSICS

FUNDAMENTALS OF ACOUSTICS.

By Lawrence E. Kinsler and Austin R. Frey. John Wiley & Sons, New York; Chapman & Hall, London. $6.00. viii + 516 pp.; ill. 1950.

This book provides a clear and concise presentation of the fundamental principles of acoustics, together with the analytical methods employed in solving acoustical problems. The first 9 chapters cover vibrations in solid bodies, acoustic waves in fluids, acoustic resonators and filters, and the absorption of sound waves. The remaining chapters deal with special topics, including: loud-speakers, microphones, psychoacoustics, architectural acoustics, underwater acoustics, and ultrasonics. Aware of the close association existing between acoustics and communications engineering, the authors, in their sections on transducers, resonators, and filters, have used a notation that emphasizes the equivalent electrical analogues of acoustical and mechanical elements. The use of the acoustical terminology proposed by the American Standards Association and the inclusion of numerous problems and tables of physical constants make the book well suited for use as a textbook in an introductory course in acoustics.

F. D. CARLSON

DESIGN FOR A BRAIN.

By W. Ross Ashby. John Wiley & Sons, New York. $6.00. ix + 260 pp.; ill. 1952.

I am sure few will dispute the statement that this is an exceptionally well written book and that the theme is developed with great care and much literary skill. This does not mean that all readers will follow the author with ease, for it is not exactly easy reading for those without some mathematical background. Nevertheless, even such individuals should not encounter too great difficulty.

The idea Ashby develops is not altogether new. There is a popular tendency today to view the brain and its function as a combination of a calculating machine, Norden bombsight, and radar-tracking device, all functioning together in technicolor. However, even though the idea isn't new, Ashby's development and contributions to the general theme are certainly valuable. He believes that a system such as the brain can be both mechanistic in nature (resembling a machine in its dependency on structural integrity and in many other ways) and also show intelligent or adaptive behavior. The essential difference between a brain and a machine is that the former makes extensive use of the principle of ultrastability. It is known that many degrees of stability can be obtained by machines such as are to be found in a number of more or less complicated calculating machines and other similar mechanical monsters that "think." But ultrastable systems are capable not only of establishing stable fields or objectives by a selective process, but also of rebuilding new fields if some parts are destroyed. To test the general reasoning put forth, Ashby built a machine, a homeostat, that fitted the definition of an ultrastable system. It was quite a remarkable gadget—he showed that any constraint or unusual complications imposed on it would make the system unstable. In turn, the instability would evoke "step function" changes until a new terminal, and stable, field was found. Also, it was shown that when this unusual constraint was removed (in this experiment a glass fiber that joined two of the magnets of the homeostat) the system became unstable again.

It is certain that there are many who will oppose an idea that explains behavior as one would explain the mechanism of action of an automatic pilot or Ashby's remarkable homeostat. It will undoubtedly be pointed out that a purely mechanistic explanation, though

helping us to understand how a system reacts correctly to environmental stresses which, if left unnoticed, would result in its destruction, usually ignores the mechanisms responsible for the subjective activities of the organ.

DAVID B. TYLER

BIOCHEMISTRY

ANNUAL REVIEW OF BIOCHEMISTRY. *Volume 21. J. Murray Luck, Editor; Hubert S. Loring and Gordon Mackinney, Associate Editors. Annual Reviews, Stanford.* $6.00. ix + 781 pp. 1952.

As usual, this volume of the *Annual Review of Biochemistry* meets the standards set by its predecessors. The editors mention in a brief preface that the earlier threat of obesity had been met in part by certain typographical changes and by the institution of new Reviews. Yet this volume is not only obese, it is pregnant. The topics presented follow: Proteolytic Enzymes (Balls and Jansen); Nonoxidative, Nonproteolytic Enzymes (Altman and Dounce); Chemistry of the Carbohydrates (Montgomery and Smith); Chemistry of the Lipids (Deuel and Alfin-Slater); Chemistry and Metabolism of Steroids (Samuels and Reich); Chemistry of Amino Acids and Proteins (Bull); Nucleic Acids, Purines, and Pyrimidines (Jordan); Fat Metabolism (Frazer); Interrelationships of Lipid and Carbohydrate Metabolism (Bloch); Metabolism of Amino Acids and Proteins (Tarver); Fat-soluble Vitamins (Kemmerer); Nutrition (Bigwood); Chemistry of Muscle (Dubuisson); Biochemistry of Neoplastic Tissue (Zamecnik); Biochemistry of Antibiotics (Work); Comparative Biochemistry (Florkin); Carotenoids (Mackinney); Alkaloids (Sargent and Small); Chromatography (Moore and Stein); Carbohydrate metabolism (Ochoa and Stern); Chemistry of Nonsteroid Hormones (Li and Harris); Water-soluble Vitamins concerned with One- and Two-Carbon Intermediates (Welch and Nichol); Biological Oxidations (Chance and Smith).

FRANCIS P. CHINARD

ACIDS AND BASES. *Their Quantitative Behaviour. Methuen's Monographs on Chemical Subjects. By R. P. Bell. Methuen & Company, London; John Wiley & Sons, New York.* $1.50. vi + 90 pp. 1952.

Two chapters sketch the historical development of concepts regarding acids and bases and the more familiar equations describing equilibrium states in aqueous solutions. The subject is broadened in Chapter III to include non-aqueous solutions made with the so-called basic, acidic, and aprotic solvents. Interionic forces are considered in Chapter IV, the relation of molecular structure to strengths of acids and bases in Chapter V, and acid-base catalysis in Chapter VI. The concluding chapter contains brief comments on "Lewis acids" and "proton acids," so renewing an old controversy that has resulted from putting new wine in old bottles and that is not confined to the terms commented on by the author.

This excellently written little monograph is of a sort that may well find a place in the library of everyone concerned with solutions. It will instruct and stimulate beginners. The advanced chemist, busy with his own particular problems, will find it a refreshing review of a subject that has broad implications.

The author does not discuss experimental techniques. He could hardly do so in the restricted space within which he has given so very good a perspective of ideas.

W. MANSFIELD CLARK

THE METABOLISM OF PROTEIN CONSTITUENTS IN THE MAMMALIAN BODY. *By S. J. Bach. University Press, New York.* $9.25. viii + 272 pp.; ill. 1952.

This book contains a comprehensive coverage of recent research concerning the metabolism of glycine, serine, threonine, valine, leucine, isoleucine, cystine, cysteine, homocysteine, and methionine in the mammalian body. A further volume dealing with the remaining protein constituents is in preparation. In general, the book is restricted to the mammal and is devoted exclusively to the metabolism rather than the chemistry of the amino acids.

Because of the unique character of glycine, considerable attention is devoted to its metabolism, including its role in the synthesis of serine, the porphyrins, and uric acid. The part played by glycine in the synthesis of glutathione, in the formation of bile salts and body proteins is also discussed. The hydroxyamino acids, serine and threonine, are presented in one chapter, with discussions of the conversion of serine into ethanolamine, phosphoserine, carbohydrate synthesis from serine, and the degradation of threonine. The metabolism of valine, leucine, and isoleucine is discussed in terms of the two general stages in their degradation, namely, deamination and oxidative decarboxylation followed by β-oxidation of the resulting acids.

The metabolism of glutathione, involving its role in the respiratory system and in the interaction with the sulfhydryl groups of enzymes, is included in the section on cystine and cysteine. The biological reduction of cystine to cysteine, the nitrogen metabolism, and the

biological synthesis of the amino acids are also adequately reviewed. Methionine is only part of the general consideration of transmethylation that includes a fairly thorough presentation of the physiological and pathological aspects of the process.

One of the major problems of an investigator in biochemistry is to keep abreast with research allied to but not directly associated with his own field. This book fulfills a need for a comprehensive coverage of amino acid metabolism in mammals which is not provided by the numerous review articles popular today. The volume, which contains over 1200 references, should prove valuable to research workers, teachers, and advanced students.

R. VAN REEN

BIOLOGICAL ANTIOXIDANTS. *Transactions of the Fifth Conference. November 30–December 1, 1950 Edited by Cosmo G. Mackenzie. Josiah Macy, Jr. Foundation, New York.* $3.75 (paper). 229 pp.; ill. 1951.

This exceedingly interesting and valuable report of the Fifth Macy Conference on *Biological Antioxidants* contains the following parts: Introductory Remarks (Paul György and Albert Lehninger); Chemical Changes Produced by Radiations (Robert S. Livingston); General Introduction to Radiation Effects on Higher Animals (Austin M. Brues); Effect of Ionizing Radiations on Sulfhydryl Systems (E. S. Guzman Barron); Genetic Effects of Irradiation and Irradiated Broths (Orville Wyss); Peroxides and Mutations (Frank Dickey); Free Radicals, Peroxides, and Antioxidants in Some Biological Processes (Elwood V. Jensen). These parts are not presented as formal papers but as informal presentations by the discussion leader together with verbatim transcripts of the discussions by those named above and the 18 other participants. The result is a rather rambling and sometimes almost unintelligible, yet always interesting consideration of the topic. There are very valuable explanations of physical and chemical relations for the biologist, and numerous compilations of data in the form of tables. Like the Oberlin Symposium on the Effects of Radiation upon Biological Systems (*Q.R.B.*, 28: 198. 1953), this makes an indispensable guide and reference work for all biologists concerned with the effects of oxygen and protective substances upon the action of irradiation on the living organism. This volume has the superiority over the Oberlin Symposium volume of being indexed. In fact, the publication contains a cumulative index for Conferences One through Five on the Biological Antioxidants.

BENTLEY GLASS

MICROBIOLOGY

TEXTBOOK OF VIROLOGY. *For Students and Practitioners of Medicine. Second Edition. By A. J. Rhodes and C. E. van Rooyen. The Williams & Wilkins Company, Baltimore.* $8.00. xv + 561 pp.; ill. 1953.

The first edition of this textbook, which appeared in 1949 (*Q.R.B.*, 26: 232. 1951), was an attempt to present a great breadth of subject matter in very concise form, primarily for the use of the medical student and practitioner. The present edition, while preserving the same general form and objective, has been extensively rewritten in less abbreviated form, information has been brought up to date, and some new material has been added. The result is a book not quite double the size of its predecessor, but still well within the size limits of the usual textbook.

This edition has been divided into 12 sections of several chapters each, the first section being concerned with fundamental characteristics of viruses and viral infection, and with methods and techniques of study. The remaining sections deal with specific diseases by groups. The book covers the viral and rickettsial infections of man and includes essential information on epidemiology, clinical features, pathology, properties of the virus, immunology, laboratory diagnosis, specific treatment (where possible), and other topics when indicated. A distinct improvement is the addition of a list of references at the end of each chapter. These refer mainly to recent publications, particularly summaries or reviews, rather than to original work. A great many illustrations have been added, and new or especially expanded topics include: laboratory diagnosis of virus diseases, coxsackie viruses, encephalomyocarditis and encephalomyelitis of animals, the recently-described Murray Valley encephalitis, and the rickettsial tick-borne fevers.

This volume is recommended as a textbook for students of medicine, bacteriology, virology, and public health. It will also be of value to medical practitioners and specialists in the various branches of medicine concerned with infectious disease.

FRANCIS B. GORDON

MICROBIOLOGY AND PATHOLOGY. *Fifth Edition. By Charles F. Carter and Alice L. Smith. The C. V. Mosby Company, St. Louis.* $5.50. 847 pp.; ill. 1953.

The fourth edition of this text was reviewed in (*Q.R.B.*, 24: 165, 1949). It is rather exceptional for microbiology and pathology to be taught together, but such a treatment offers many advantages in the instruction of nurses and laboratory technicians. For institutions where the two subjects are taught conjointly this book

will merit careful consideration. A number of modernizations has been made in the treatment since the previous edition. The tests for readers, and the references following each chapter have been retained. For an elementary approach, the work can be highly recommended. Incidentally, the present review copy is very well printed, a great improvement over the miserable printing job in fourth edition.

WALTER C. TOBIE

AN ILLUSTRATED GUIDE FOR INTRODUCTORY BACTERIOLOGY.

By R. M. Thomas and J. E. Prier. Burgess Publishing Company, Minneapolis. $3.00. iii + 94 pp.; ill. 1952.

This is another manual in offset printing with wire binder. It is well reproduced, and full of useful elementary material. Its object is stated in the introduction to be to provide illustrated laboratory material for students who have "some difficulty in grasping the essentials of technic by reviewing word descriptions." It fulfills this purpose well, but it seems probable that lecture demonstrations would serve the purpose even better. Pictures are often much better than "word descriptions," but actual working demonstrations (if it is at all possible to give them) are still more effective. There are 41 exercises, usually with tabular spaces where the student's results can be filled in.

WALTER C. TOBIE

LABORATORY MANUAL FOR MICROBIOLOGY.

By Catherine Jones Witton. McGraw-Hill Book Company, New York, Toronto, and London. $2.00. viii + 76 pp. 1952.

The pages of this well-printed manual have a line of fine perforations along the inside edge so that they can be torn out and inserted in a student's notebook of the ring-binder type by means of larger triple perforations. The manual is to be used as an adjunct to the author's book *Microbiology with Applications to Nursing*. In an elementary course for nurses, the manual should serve this purpose quite well.

WALTER C. TOBIE

PRACTICAL EXPERIMENTS IN GENERAL BACTERIOLOGY.

By Rufus Clay Hatfield. Wm. C. Brown Company, Dubuque. $2.75. vi + 104 pp. + 1 chart; ill. 1952.

Among the large output of laboratory manuals of bacteriology in lithoprint or offset process, with spiral wire binders, the present work appears to be rather superior. Besides containing rather more material than is usual in this type of manual (50 exercises) it is well-written and well-organized. It deals with bacteriology in the strict sense, rather than dealing with general microbiology. Spaces are left for the insertion of laboratory results. The pages are so prepared that they can be torn out and inserted in a laboratory notebook with three-ring binders. Harassed teachers of bacteriology will quite possibly find this an outstanding work.

WALTER C. TOBIE

SANITARY GERMS.

By Emil F. Tiesen. Bruce Humphries, Boston. $2.50. 95 pp. 1951.

The subtitle on the dustcover states that this is: "An illuminating handbook debunking the germ theory—written in everyday language by a well-known chiropractor." Two quotations will give the flavor of the work: "The sulfa-drugs, penicillin, streptomycin, hystomines [sic!], are all produced from filthy mold, differing only in the kind of soil or dead material from which they are obtained" (p. 74). "Germs are the best friends we have; in fact they are the very food we eat. Our whole body is made up only of germs and water; that's all it is; with the spirit living inside us as tenant and manager" (p. 50). These samplings should suffice.

WALTER C. TOBIE

MEDICAL MYCOLOGY. *An Introduction to its Problems. By G. C. Ainsworth. Pitman Publishing Corp., New York, Toronto, and London.* $2.75. ix + 105 pp. + 8 pl.; text ill. 1952.

Ainsworth, who speaks with the authority of an experienced and careful mycologist, is one of the men most responsible for a revival of interest in medical mycology in England. He has prepared in this little book an introduction to that subject which does more than list the mycoses and the fungi which cause them. As suggested by the subtitle, the book is a stimulating presentation of some of the practical and theoretical problems which are in varying degree peculiar to the mycoses.

The introductory chapter is a concise discussion of the medical importance of the mycoses, the terminology of mycology, relationships of the fungi causing human disease, the geographical distribution of mycoses and certain characteristics of selected mycoses. The remaining 7 chapters take up specific mycoses and the particular problems which they or the fungi causing them typify.

The dermatophytoses are the most important of the mycoses in respect to clinical types, incidence, and wide geographic distribution. Correlated with these characteristics are the variability and multiplicity of species

of the fungi causing these superficial skin infections. Chapter II therefore outlines some of the epidemiological aspects of dermatophytosis and the taxonomic and nomenclatorial problems posed by this large group of closely related fungi.

In the third chapter the troublesome question of the etiologic importance of saprophytic fungi which are capable of becoming pathogenic under suitable conditions is illustrated by a discussion of *Candida albicans* and related species. The author discusses the differential characteristics of the important species of *Candida* and the occurrence of these fungi as secondary invaders in pulmonary disease.

The problem of "multiple causation," which to some extent complicates the orderly presentation of medical mycology at the textbook level, is illustrated by a discussion of mycetoma ("Madura foot") which is a clinical syndrome caused by one of many fungi (the author lists 35) as distantly related as *Nocardia* and *Allescheria*. Chromoblastomycosis is also cited as an example of a mycosis in which 3 or more species of fungi having obvious close relationships present an unusual problem in nomenclature.

The principal systemic mycoses are cited to illustrate the importance of dimorphism and the saprophytic occurrence of the etiologic fungi. Although direct transmission of these systemic mycoses is theoretically possible, it does not appear to be clinically important. The importance of man's exposure to spores of the saprophytic phases of these pathogenic fungi, and the isolation of these fungi from soil and other sources in nature, are properly stressed.

Chapter VI briefly reviews the general fields of immunity and serologic diagnosis, mentions methods of preparing some fungus antigens, and discusses the serologic diagnoses of mycoses in the light of cross reactions due to the occurrence of antigens common to more than one species of fungus.

Spores of saprophytic fungi are among the allergens which can sensitize man. There is a brief historical review of studies of allergic reactions induced by spores of fungi, reference to some of the more important allergenic molds, and a discussion of the significance of molds in air and dust. The final chapter is on poisonous mushrooms, types of toxins, and ergotism.

The book is illustrated by a few figures and well-chosen plates. It follows a new and interesting pattern in orienting the student into an important field of medical mycology.

C. W. EMMONS

A BACTERIOLOGICAL STUDY OF AUTOMATIC CLOTHES WASHING.

By G. M. Ridenour. The National Sanitation Foundation, Headquarters School of Public Health, University of Michigan, Ann Arbor. 50 cents (paper). viii + 116 pp.; ill. 1952.

This is a very extensive and detailed study of the effects of washing and laundering (with and without germicidal agents) upon the bacterial content of fabrics. It will be of undoubted value and interest to those concerned with this field. An unusual finding is that quaternary ammonium compounds are effective against spore-forming organisms. Under the usual laboratory test conditions, these compounds have little action against sporogens or spores. Their greater activity in this study probably results from the use of different conditions and test methods.

WALTER C. TOBIE

TERRAMYCIN. *Ann. N. Y. Acad. Sci., Vol. 53, Art. 2. September 15, 1950.*

Edited by Roy Waldo Miner; C. S. Keefer, Conference Chairman. New York Academy of Sciences, New York. $3.00 (paper). Pp. 221–460 + 4 pl.; text ill. 1950.

There is presented in this volume a comprehensive survey of the chemical and physical properties of terramycin, its pharmacology, a review of absorption, excretion, and distribution studies, its antimicrobial action, and clinical studies of its use. The individual chapters were written by experts in the field, and as usual the volume meets the high standards of excellence that have been established by previous surveys of the New York Academy of Sciences.

C. JELLEFF CARR

PARASITOLOGY

FLEAS, FLUKES AND CUCKOOS. *A Study of Bird Parasites.*

By Miriam Rothschild and Theresa Clay. Philosophical Library, New York. $8.75. xiv + 304 pp. + 40 pl.; text ill. 1952.

Prepared by specialists on bird-fleas and bird-lice, respectively, this handsome little volume is a curious mixture of broad natural history and tight technicality. Most of the former occupies the first 55 pages, on general considerations of parasitism, commensalism, symbiosis, interactions between parasites and hosts, and the development of parasitism. Part Two—the next hundred pages—deals with the authors' specialties and illustrates "the receptaculum seminis of female British bird fleas and mammal fleas" to support the textual accounts. The remainder of the book covers other parasites of British birds, including the faunas of birds' nests. Two brief final chapters discuss the pirati-

cal skua and the parasitic European cuckoo as organisms feeding and raising young at the expense of other birds. A bibliography and fine indexes complete the book. It is Special Volume 7 in the "New Naturalist" Library series.

LORUS J. & MARGERY J. MILNE

BIOLOGIE D'ANOPHELES GAMBIAE. *Recherches en Afrique-Occidentale Française. Organisation Mondiale De La Santé, Serie de Monographies No. 9.*

By M. H. Holstein. World Health Organization, Genève. $2.00 (paper). 176 pp.; ill. 1952.

Malaria is a serious economic and public health problem in Africa. The use of the modern residual sprays, directed against *Anopheles gambiae*, the principal vector, has not always produced satisfactory results. The author emphasizes that effective control must be based upon a thorough knowledge of the vector species, and in this monograph he presents the results of his extensive studies on *A. gambiae* in French West Africa. The author discusses the morphology and taxonomy of *A. gambiae* and its variants, the life cycle, breeding habitats, influence of environmental factors on survival of the species, adult habits, and the relative importance of *A. gambiae* and *A. funestus* as malaria vectors. He concludes that there are two populations of *A. gambiae*, one of which feeds especially upon animals; this one has a higher maxillary index and breeds by preference in waters with a high content of dissolved organic material. The other, with fewer maxillary denticles, is especially attracted to man, and breeds in water with a lower content of organic matter. Although we think of *A. gambiae* as being a highly domestic mosquito, Holstein has found a high proportion of the anthropophilic strain resting outside of inhabited dwellings. This may be a clue to the lack of success of some residual spray programs.

There has been a great deal of research on *A. gambiae*, and other extensive reviews of the biology of this species have been written. This one merits the attention of malariologists and others who have an interest in the biology and control of mosquitoes. Some of us may not agree with the importance Holstein places upon the maxillary index; however, his ideas are thought-provoking and should stimulate additional research.

L. E. ROZEBOOM

CLINICAL PARASITOLOGY. *Fifth Edition, thoroughly revised.*

By Charles Franklin Craig and Ernest Carroll Faust; with a chapter by Albert Miller. Lea & Febiger, Philadelphia. $12.00. 1032 pp. + 6 pl.; text ill. 1951.

Previous editions (see *Q.R.B.*, 13: 242. 1937) of this standard American textbook in human parasitology have enjoyed widespread use by students and advanced workers in the field. Since the present edition is, for the most part, a verbatim transcript of the most recent previous edition (1945), and in any case is organized along precisely the same lines, no extended description appears to be necessary.

The book is divided into the sections Protozoa, Helminths, and Arthropods. Under each heading the various species are dealt with according to taxonomic position. Sections devoted to techniques and to the literature of the subject complete the presentation.

As in the past, life cycle diagrams and tables comparing various members of a group are the exception rather than the rule. However, the text is well organized under paragraph headings, and specific information is not difficult to find. Literature citations have been brought up to 1949–50, but it is surprising how little has had to be changed or added to the great mass of material already accumulated in the previous edition. Notable exceptions, of course, are to be found in the field of malaria with the demonstration of the pre-erythrocytic stages in human malaria, the development of new drugs for treating malaria and filariasis, many new data on practically all aspects of schistosomiasis, and lastly, the development of the residual insecticides. The last development has had such an impact on the whole field of insect control that an entirely new chapter, The Control of Medically Important Arthropods, by Albert Miller, has been added.

This edition retains both the strength and weaknesses of the last. It presents a wealth of information concisely, but controversial subjects tend to be presented dogmatically from the authors' standpoint only. For example, the statement is made that "Thus, strictly speaking, a healthy carrier of *E. histolytica* does not exist . . .," in spite of the considerable body of opinion to the contrary. This edition, also, perpetuates as fact the questionable theory that "Moses, who received medical instruction from the Egyptian priests, prescribed sanitary laws to guard against plagues from insect pests as well as from infected animal flesh."

From the standpoint of legibility, this edition is slightly better than its predecessor, 49 lines having been put on the same sized page that formerly carried 51. However, this is still considerably more print per page than is to be found in any of several textbooks picked up at random, and is still a little hard on the eyes.

Minor imperfections notwithstanding, this is a valuable book which should be within easy reach of all workers in the field of medical parasitology.

MORRIS GOLDMAN

HEALTH AND DISEASE

COLOR ATLAS OF PATHOLOGY.

Prepared under the auspices of the U. S. Naval Medical School of the National Naval Medical Center, Bethesda, Maryland. J. B. Lippincott Company, Philadelphia, London, and Montreal. $20.00. xiv + 546 pp.; ill. No date.

This expensive book contains many colored illustrations of micropathological preparations, as well as numerous vividly colored photographs of gross specimens. There is a short clinical summary for each of the 1053 illustrations, while a summary of pathological data of certain system diseases is presented in almost outline form. The resulting volume forms an interesting colored atlas of selected pathological material, but in no way provides a substitute for the conventional textbook of pathology.

ELLA H. OPPENHEIMER

ATLAS OF HISTOLOGIC DIAGNOSIS IN SURGICAL PATHOLOGY.

By Karl T. Neuberger, with a section on Exfoliative Cytology by Walter T. Wikle; photography by Glenn F. Mills. The Williams & Wilkins Company, Baltimore. $11.00. x + 460 pp.; ill. 1951.

This volume consists mainly of half-tone reproductions of low and medium power micrographs of surgically obtained pathological sections. In the main section and supplement each of the 838 figures, measuring about 3 x 3½ in., is accompanied by brief reference to pertinent histological, clinical, and gross features of the case. Coverage is general with the exception of bone marrow. There is no attempt to grade malignancies. The section on exfoliative cytology contains 42 figures, each with one or more high-power micrographs, with descriptive titles. The micrographs are of good quality. But they are subject to the limitations always set by black and white reproductions of stained preparations, the interpretation of which depends largely on color.

The purpose of this volume is to present pictures of histopathological conditions to those who must diagnose them from actual specimens. Its usefulness to others is naturally very limited.

F. N. LOW

AUTOPSY DIAGNOSIS AND TECHNIQUE. *Third Edition, Revised and Enlarged.*

By Otto Saphir; Foreword by Ludvig Hektoen. Paul B. Hoeber, Medical Book Department Of Harper & Brothers, New York. $6.00. xxiv + 471 pp.; ill. 1951.

This is a valuable book for the young pathologist and serves primarily as an aid to gross diagnosis at the autopsy table. This third edition (2nd ed., *Q.R.B.*, 22: 93. 1947) contains up-to-date descriptions of the gross changes recognized in all types of disease. The instructions in orderly dissection and systemic examination should also prove of great value to the biologist in performing autopsies on his experimental animals. In addition to gross pathological changes, this volume includes tables of normal body and organ weights and measurements which are virtually indispensable as ready reference tables for the experienced as well as the novice pathologist.

ELLA H. OPPENHEIMER

MUIR'S TEXT-BOOK OF PATHOLOGY. *Sixth Edition. Revised by D. F. Cappell. The Williams & Wilkins Company, Baltimore.* $10.00. xx + 1090 pp.; ill. 1951.

The sixth edition of *Muir's Text-Book of Pathology* has been revised by Cappell, following the pattern of the previous editions. General disease processes are first discussed and followed by a review of system diseases. Many of the chapters are excellent, but due to the fact that this was published in 1951, recent information on a variety of subjects (arteriosclerosis, gout, glycogen storage disease, and others) is lacking. The bibliography is limited mostly to British references and to general rather than specific subjects. Although this volume is a standard British textbook of pathology, it cannot be recommended as superior to the numerous recent American textbooks now used in our medical schools.

ELLA H. OPPENHEIMER

PHYSICS AND MEDICINE OF THE UPPER ATMOSPHERE: A STUDY OF THE AEROPAUSE. *Proceedings of a Symposium on the Physics and Medicine of the Upper Atmosphere held at San Antonio, Texas, November 6–9, 1951.*

Edited by Clayton S. White and Otis O. Benson, Jr. University of New Mexico, Albuquerque Press (for the Lovelace Foundation for Medical Education and Research). $10.00. xxiv + 611 pp.; ill. 1952.

This volume documents data that were presented at a symposium held in 1951 by the USAF School of Aviation Medicine and The Lovelace Foundation for Medical Education and Research. The subject was the possibility of manned travel in the upper atmosphere. This report, according to Dr. C. S. Clayton, Director of Research for The Lovelace Foundation, is concern: with a continuation of Phase I of the Project, which is to select, collect, and document available data from the "open" literature and to set forth all the foreseeable medical and physical problems. The result is a number

of contributions from some 34 scientists cross-sectioning at least 4 scientific specialties, such as astrophysics, aeronautical engineering, radiobiology, and aviation medicine. It may be said with safety that: (1) the book is nicely gotten together; (2) the illustrations and photographs, some of which are fascinating, make for easy reading; (3) it does not contribute anything new and startling in the way of new knowledge; and (4) no aid and comfort is given to the enemy by any possible leak of "top secret" information. This last is a surmise based on the fact that a very popular weekly magazine has recently published a series of articles along very similar lines and covering much of the material presented here.

DAVID B. TYLER

PRINCIPLES AND PRACTICE OF AVIATION MEDICINE. *Third Edition.*

By Harry G. Armstrong. The Williams & Wilkins Company. $7.50. x + 476 pp. + 1 pl.; text ill. 1952.

A new edition of a very popular textbook in which many chapters have been revised to include a summary of much of the work done since 1943.

COLD INJURY. *Transactions of the First Conference, June 4–5, 1951, New York.*

Edited by M. Irene Ferrer. Josiah Macy, Jr., Foundation, New York. $3.25. 248 pp. + 2 pl.; text ill. 1952.

This first Macy conference on Cold Injury includes a report of the discussions of the types of animal studies being carried out in the various laboratories and the present ideas that are held concerning homokinesis, hypothermia, and acclimitization.

THE PATHOLOGY OF DIABETES MELLITUS. *Third Edition.*

By Shields Warren and Philip M. LeCompte. Lea & Febiger, Philadelphia. $7.50. 336 pp. + 3 pl.; text ill. 1952.

A very well organized monograph prepared by two workers who know their subject and how to present it. In this textbook an attempt is made to understand the abnormalities of function in the diabetic patient and to relate them to changes in the structure of the organ and cell. Attention is paid to the advances made in the field of experimental diabetes, the changes encountered in infancy and childhood and in pathology, particularly of the kidney, eye, and endocrine glands.

DAVID B. TYLER

DIABETIC GLOMERULOSCLEROSIS. *The Specific Renal Disease of Diabetes Mellitus.*

By Harold Rifkin, Louis Leiter, and James Berkman. Charles C Thomas, Springfield, Ill. $3.50. ix + 102 pp., ill. 1952.

This monograph presents an excellent review of the clinical and pathological aspects of diabetes. It stresses especially the renal vascular lesions found in diabetes as well as the generalized vascular pathology of the diabetic. Theories of pathogenesis of these lesions are discussed and the pathological material is correlated with the clinical data available at the Montefiore Hospital with special reference to age of onset of the disease. Chapters are devoted to laboratory aids in diagnosis, differential diagnosis, and treatment. There is an excellent bibliography.

ELLA H. OPPENHEIMER

THE LIVER. *Porta Malorum (The Gateway to Disease).*

By Kasper Blond and David Haler. The Williams & Wilkins Company, Baltimore. $5.00. viii + 268 pp.; ill. 1950.

This book sets forth, and attempts to document, the idea that apparently diverse states such as piles, varicose veins, cholecystitis, nephritis, jaundice, ulcerative colitis, and allergy, to name a few, have as a common factor a malfunctioning liver. The authors emphasize the importance of the venous system in the causation of many of these poorly understood conditions and try to explain the symptoms produced by liver damage bringing about a portal hypertension which, in turn, obstructs venous return—particularly since not every individual has adequate open shunts between the portal and the caval systems.

Undoubtedly, some of the suggestions put forth here will remind some critical readers of the ancient Babylonian group of priestly doctors known as hepatoscopists. Nonetheless, this is a scholarly and stimulating monograph, well written, illustrated, and indicative of a wide experience. It contains a good bibliography and is adequately indexed.

DAVID B. TYLER

MEDICAL BIOGRAPHIES. *The Ailments of Thirty-three Famous Persons.*

By Philip Marshall Dale. University of Oklahoma Press, Norman. $4.00. xii + 259 pp. + 28 pl. 1952.

The history of medicine and surgery is probably nowhere more interestingly recorded than in the medical biographies of the 31 world-famous persons covered in this volume. Post-mortem diagnoses, particularly in the absence of clinical records or autopsy findings, are

never easy; and when the source of information is limited to the written word of someone other than a physician, and propagated over many centuries, with all the vagaries of translation and rewriting, the task becomes formidable indeed. However, the author has combined a thorough appreciation of history with a high degree of clinical perception and a vivid sense of humor to produce a completely enjoyable treatise.

Beginning with Buddha (563–493 B.C.), Charlemagne (742?–814), and William the Conqueror (1027/28–1087), the author has included subjects representative of every century from the 15th to the present—William McKinley and Grover Cleveland both having died since 1900. Although the information is sometimes fragmentary, the writer has included whatever material is available on the progenitors and other close relatives of the person under consideration. In addition to the circumstances surrounding the demise of each subject, something of his ailments throughout life are included. In the light of present medical and surgical knowledge and practice, it is apparent that many errors in diagnosis and much unwarranted suffering were associated with the illnesses of these famous people of the past, but then it must be remembered that the present level of attainment in medicine and surgery could have been reached only as a result of the illnesses and the suffering, not only of the great, but of the ordinary people of the past. The appended list of references is extensive.

B. AUBREY SCHNEIDER

RHEUMATIC FEVER. *A Symposium held at the University of Minnesota on November 29, 30, and December 1, 1951, under the sponsorship of the Minnesota Heart Association.*

Edited by Lewis Thomas. University of Minnesota Press, Minneapolis. $10.00. vii + 349 pp. ill. 1952.

The 23 articles presented in this symposium emphasize the fact that rheumatic fever is still a mystery as to cause, cure, and prevention. Some progress has been made, perhaps, in preventing cardiac damage through prompt treatment and prophylaxis of the streptococcal infections that seem to incite rheumatic fever and its recurrences. However, as stressed in the sections on epidemiology, it is still a most important cause of disability and premature death.

The participants in this symposium were concerned with streptococcal infections almost to the exclusion of other possible causes. Indeed, this volume would have been more suitably entitled "Streptococcosis and its relationship to Rheumatic Fever." The onset and recurrence of this disease are interpreted as arising from infection with any of the large number of serologic types of group A streptococcus. The articles on histopathology and microbiology present a great deal of information on the streptococcal cell and its components, and the tissue reaction in rheumatic fever and streptococcal infections. Unfortunately, these observations do not explain the disease. The reports on immunology and serology indicate that a greater antibody response occurs in patients who develop rheumatic fever following streptococcal infections than in patients with uncomplicated streptococcal infections.

Attempts to induce rheumatic fever in experimental animals continue to fail, altho the Arthus and Schwartzman phenomena produce tissue changes that are tantalizingly similar. The biochemical researches reported here have as yet failed to cast light on the specific disease process.

In the final section, a good deal of hope for prevention is offered. Antibiotics prevent recurrence of streptococal infection and the outward manifestations, at least, of rheumatic fever. It is not yet certain, however, that their use will prevent rheumatic heart disease. Treatment is discussed from the standpoint of general management, of antibiotics, and of drugs such as ACTH, cortisone, and the old standby—aspirin. Surgery of the heart valves can provide help in some advanced cases.

This volume presents a wealth of interesting material for the average reader with general biologic and scientific interests, and some provocative thoughts for the person working in closely related fields. It is, however, unfortunate that the symposium has been limited almost entirely to the relationship of streptococci and rheumatic fever. It seems possible, indeed likely, that streptococcosis is only a trigger mechanism in the production of rheumatic heart disease, and not necessarily the only one. The genetic aspects and other fields of investigation that may be of great importance in addition to or possibly instead of streptococci have not been adequately represented. Until complete understanding is attained, it seems unwise to focus on one possible cause to the virtual exclusion of other factors.

PAUL A. LEMBCKE

DIAGNOSTIC AND EXPERIMENTAL METHODS IN TUBERCULOSIS. *Second Edition.*

By Henry Stuart Willis and Martin Marc Cummings. Charles C Thomas, Springfield, Ill. $10.00. xxxv + 373 pp.; ill. 1952.

The second edition of this book has been completely rewritten, incorporating advances in the diagnosis and therapy of tuberculosis. It contains excellent descriptions of the laboratory procedures for diagnosis and experimental study of tuberculosis. The tubercle bacillus, its detection by staining, cultivation, and animal inoculation, and the abnormal body fluids resulting from its presence in the body are described. There are chapters devoted to histological techniques, BCG vac-

cination, antibiotics and tuberculin, as well as a summary of tuberculous infection in experimental animals. The volume was written as a laboratory approach to tuberculosis for practicing physicians, public health officials, laboratory workers, pathologists, and medical students. It fulfills this purpose in admirable fashion.

ELLA H. OPPENHEIMER

GLOBAL EPIDEMIOLOGY. *A Geography of Disease and Sanitation; Africa and the Adjacent Islands, Vol. II. By James Stevens Simmons, et al. J. B. Lippincott Company, Philadelphia, London, and Montreal.* $15.00. xl + 652 pp.; ill. 1952

This is the second volume of an encyclopedic series. The first, published in 1944, covered India, the Far East, and the Pacific area. A very brief introduction points out the great diversity of peoples, languages, and customs in the African continent, and the generally low levels of basic sanitation, nutrition, education, standards of living, and medical care. The 45 national or colonial units covered are grouped in 8 sections: The Nile Valley, Ethiopian Highlands, East Africa, Islands of the Indian Ocean, South Africa, Equatorial Africa, West Africa, and Northern Africa. It would perhaps have been helpful, especially to the reader with broad interests, if a brief summary of the major factors of disease, and outstanding similarities and dissimilarities, could have been provided for each of the major geographic regions. As it is, they are held together only by regional maps

Each of the 45 units is described concisely and systematically in an average of 12 pages, and never more than 24. The information for each is presented in narrative style, marshalled into 5 main groups: (1) geography and climate; (2) population, vital statistics, social economy, food and nutrition, and housing; (3) water supplies, waste disposal, fauna and flora, and food sanitation; (4) health organization, medical institutions, and personnel; and (5) diseases spread or contracted chiefly through intestinal or urinary tracts, respiratory tract, through contact, by arthropods, and nutritional diseases. General medical conditions such as diabetes, disorders of the heart, of cancer, are not mentioned, but diseases of environmental or industrial interest, such as silicosis or lead poisoning, are listed under a miscellaneous heading. The information is summarized briefly and an extensive bibliography is furnished.

This book is highly recommended as an extremely valuable source of information for anyone desiring a brief and comprehensive picture of medical, health, and sanitary conditions in any of the various divisions of Africa, and the fine bibliography would be helpful in acquiring more detailed knowledge.

PAUL A. LEMBCKE

ANOPHELES AND MALARIA IN THE NEAR EAST. *London School of Hygiene and Tropical Medicine, Memoir No. 7.*

By H. S. Leeson (Anopheline Surveys in Syria and Lebanon); W. H. R. Lumsden and J. Yofe (Anophelism and Malaria in Transjordan and in the Neighbouring Parts of Palestine and Syria); and T. T. Macan (The Anopheline Mosquitoes of Iraq and North Persia): Introduction by P. A. Buxton. H. K. Lewis & Company, London. 35 s. xii + 223 pp. + folded table; ill. 1950.

This monograph is based upon surveys conducted by Malaria Field Laboratories of the British A y during World War II.

The area in which the surveys covered in the three separate reports were made is hot and dry, and the existence of intense malaria in it would seem to be rather paradoxical. Much of the malaria is man-made. Water for agricultural purposes is obtained from rivers or from subterranean sources, and is distributed through irrigation channels. Misuse of such water creates the breeding places for the vector species.

Malaria has not yet ceased to be a public health problem, in spite of the great advances which have been made in its control through the use of residual insecticides. These reports should be of considerable interest to malariologists, medical entomologists, and others who are concerned with the disease and with mosquitoes. Our knowledge of the epidemiology of malaria, and of the species, distribution, and habits of the *Anopheles* mosquitoes of the Near East has been incomplete. This monograph supplies the information which has been lacking, although, as the authors themselves recognize, there remain problems yet unsolved, particularly with respect to the biology of the mosquitoes.

L. E. ROZEBOOM

YELLOW FEVER.

Edited by George K. Strode and John C. Bugher, J. Austin Kerr, Hugh H. Smith, Kenneth C. Smithburn, Richard M. Taylor, Max Theiler, Andrew J. Warren, and Loring Whitman. McGraw-Hill Book Company, New York, Toronto, and London. $9.50. xv + 710 pp.; ill. 1951.

This book tells the story of the part played by the International Health Division of the Rockefeller Foundation in the conquest of yellow fever. When the Yellow Fever Commission of the Rockefeller Foundation was organized in 1916, the epidemiology of the disease was thought to involve a simple man → *Aedes aegypti* → man transmission of the causative agent. Three requirements for the maintenance of the disease were adequate numbers of active cases, of *A. aegypti*, and of non-immunes. These conditions were met only in large urban centers of population in the tropics, which

served as foci of infection from which yellow fever could spread to other areas. Gorgas had demonstrated that yellow fever could be eradicated from large cities through the control of the mosquito vector; hence it was assumed that an attack on the mosquito in key centers of yellow fever would lead eventually to the eradication of the disease from the world.

Today yellow fever is still with us. During the years intervening since the Rockefeller Foundation first organized its campaign, cooperative studies by doctors, entomologists, and other biologists have established the fact that yellow fever exists endemically throughout the large forested areas of tropical America and Africa, where it is maintained by passage especially through monkeys and mosquitoes which inhabit the forest canopy. However, although the original objective of eradication has not been realized, the disease is no longer the scourge it once was, as adequate means of protection of man are now available to us.

One can learn again from the book that "pure" research may have intensely practical applications. The laboratory studies on the etiologic agent showed that it is a filterable virus, and that not only man, but a number of species of primates are susceptible. Furthermore, the virus was adapted to mice through intracerebral passage; this led to the mouse protection test whereby the geographic distribution of the disease could be determined. Mosquitoes other than *A. aegypti* were shown to be efficient vectors; this contributed to the eventual elucidation of the complicated transmission cycle in the tropical forests. Propagation of the virus in tissue culture resulted in the appearance of a modified strain which has been utilized for mass vaccination.

The cost to the Rockefeller Foundation of the campaign totalled about 14 million dollars; it is estimated that about 36 per cent of this money was spent in research. Six of the 76 staff members who participated in the program died of yellow fever.

This book is divided into 10 sections, each of which is written by an author who is particularly qualified by reason of his experience with the disease while he was a member of the staff of the International Health Division. Thus, although there are still some unsolved problems with respect to the survival of yellow fever, so that the last chapter in its history still remains to be written, the book presents a thorough coverage of our present state of knowledge of yellow fever. Biologists, medical people, and many laymen will find it to be intensely interesting.

L. E. ROZEBOOM

YELLOW FEVER IN GALVESTON, REPUBLIC OF TEXAS, 1839. *An Account of the Great Epidemic. By Ashbel Smith; with a biographical sketch by Chauncey D. Leake, and stories of the men who conquered yellow fever. University of Texas Press, Austin.* $2.50. xx + 135 pp. + 3 pl. 1951.

Ashbel Smith received his M.D. from Yale in 1828. After practicing medicine for a while in North Carolina, he moved to Texas, where he became a prominent political figure from the time Texas first existed as a Republic until after the Civil War. He was appointed Surgeon General of the Republic of Texas in 1837. Although busy with civic affairs, Smith maintained his medical practice. The bulk of this little book is a reprint of Smith's original pamphlet, first published in 1839, on the yellow fever epidemic of 1839 in Galveston.

During the epidemic, Smith attended many of the cases and performed a number of autopsies. This experience enabled him to write a first-hand account of the symptomatology and pathology of the disease. In the preface to the present edition Chauncey Leake points out that the essential clinical information provided by Smith has been improved upon significantly in only two ways: the determination of the causative agent and its mechanism of transmission.

At the present time, this book can contribute little to our knowledge of yellow fever. However, it is of historical interest, and the historical value is enhanced by a series of short biographical sketches, most of which were prepared by Josiah C. Trent, of the men who were pioneers in the conquest of yellow fever.

L. E. ROZEBOOM

VIRUSES AS CAUSATIVE AGENTS IN CANCER. *Ann. N. Y. Acad. Sci., Vol. 54, Art. 6. By Roy Waldo Miner, Editor; C. P. Rhoades, Consulting Editor; and 45 Others. New York Academy of Sciences, New York.* $4.00 (paper). Pp. 867–1232; ill. 1952.

Since the demonstration by Rous in 1911 of transmission of a fowl tumor by cell-free filtrate, information has been accumulating on the association of certain tumors with viruses or virus-like factors. Of even greater significance is the increasing indication, from basic studies on the effect of viruses upon their host cells, that the two areas of study, oncology and virology, have much in common at the cellular level. A consideration of this relationship by a panel of authorities in their respective fields provides an interesting and timely review of the subject.

The series of 31 papers assembled in this volume is the result of a conference held by the Section of Biology of the New York Academy of Sciences, on November 16 and 17, 1951. The scope of the conference was broader than is indicated by the title. The series of papers includes 3 concerning studies on bacteriophages, 5 on pathologic or immunologic studies on viruses, and 3 reporting observations made by electron microscopy.

Most of the remaining papers deal with various types of neoplasms that are associated more or less directly with viruses. These include tumors of mammals, fowls, fish, amphibians, and plants. One chapter is devoted to viruses with oncolytic properties and their adaptation to tumors.

FRANCIS B. GORDON

STUDIES IN BRUCELLOSIS, III. *A Series of Five Papers. Memoir 6.*

By I. Forest Huddleson, Marvis A. Richardson, Juanita Warner and Betty Baltzer. Michigan State College Agricultural Experiment Station, Dept. of Bacteriology and Public Health, East Lansing. $1.00. 114 pp. + 6 pl.; text ill. 1952.

The 5 subdivisions of this monograph deal respectively with: dissociation in the genus *Brucella* (in general); dissociation in Type G (microcolony type) of *B. abortus*; antigenic properties of different colony types of the genus *Brucella*; differentiation of species and strains of species using tetrazolium salts; and a factor in bovine serum and colostrum which inhibits *Brucella*. There are excellent reproductions of color photographs of different types of colonies. Each paper is followed by a series of references.

It is somewhat difficult to evaluate and review a work as technical as this one. Specialists in the field will find it of great value to consult this monograph and make their own evaluations.

WALTER C. TOBIE

MONOGRAPHS IN MEDICINE. *Series I.*

Edited by Wm. B. Bean; Associate Editors, Morton Hamburger, John A. Leutscher, and Stewart Wolf. The Williams & Wilkins Company, Baltimore. $12.00. ix + 655 pp.; ill. 1952.

This is the first of a proposed series of volumes that will attempt a "... compromise between the journal focused on the temporary present, and the fully comprehensive volume that purports to cover the entire field of knowledge." The purpose is admirable and the individual monographs are good, but the series is not likely to attract readers until the editor can furnish a comprehensive outline for the whole series. As will be seen in the list below, the 15 monographs in Series One have no logical relationship to each other, and one is given no clear indication of what is to follow in future volumes nor definite assurance that there will be subsequent issues. It also seems to me that these monographs, which range from 20 to 70 pages in length, would be more useful and more widely read if published either as individual volumes or in a comprehensive series, perhaps as a loose-leaf system.

The monographs are on the whole excellent, clear, concise, and amply documented. The only fault to be found is a minor one, consisting of a few errors in the numbering of bibliographic references. The subjects and authors are as follows:

Talking with the Patient (Stewart Wolf); Precordial Noises Heard at a Distance from the Chest (William B. Bean); Physiology of the Body Fluids (William W. Wallace); Angiocardiography (Charles T. Dotter and Israel Steinberg); Portal Hypertension (Mary Ann Payne and Charles G. Child, III); Pheochromocytoma (Henry Aranow, Jr.); Respiratory Failure in Neuromuscular Disorders (Fred Plum); Cortisone and ACTH in Infectious Processes (Max Michael, Jr.); Prevention of Rheumatic Fever (Charles H. Rammelkamp, Jr. and Floyd W. Denny); Amebiasis (Henry E. Hamilton); The Present Status of the Chemotherapy of Human Malaria (L. H. Schmidt); The Seasonal, Arthropod-borne, Virus Encephalitides (R. Walter Schlesinger); Sickle Cell Anemia (Byrd S. Leavell and William A. MacIlwaine); The Growth and Maturation of the Erythrocyte (Richard W. Vilter and John F. Mueller); Chemical Agents Used in the Treatment of Inoperable and Far-advanced Neoplastic Disease (David A. Karnofsky).

PAUL A. LEMBCKE

PSYCHOLOGY AND ANIMAL BEHAVIOR

ANNUAL REVIEW OF PSYCHOLOGY, 1952. *Vol. III. Edited by C. P. Stone. Annual Reviews, Stanford.* $6.00. x + 462 pp. 1952.

This is the third in a series of volumes similar to the older annual review series in biochemistry and in physiology. Like its two predecessors, it is an extraordinarily useful reference work, to members of related professions almost as much as to psychologists themselves. Each chapter deals with one of the arbitrary subdivisions within psychology, summarizing, integrating, and briefly discussing the work on each of the many problems within each field during recent (particularly the post-war) years. Each year's review generally covers topics somewhat different than its predecessor, save in those fields, such as vision and personality, in which the literature grows so rapidly as to require yearly discussion.

The material discussed is divided into 17 chapters as follows: Child Psychology; Learning; Vision; Hearing; Somesthesis and the Chemical Senses; Individual Differences; Personality; Social Psychology and Group Processes; Industrial Psychology; Comparative and Physiological Psychology; Abnormalities of Behavior; Clinical Methods: Psychodiagnostics; Clinical Methods: Psychotherapy; Counseling: Therapy and Diagnosis; Educational Psychology; Statistical Theory and Research Design; and Motivation.

The chapters on vision (Harry Helson), hearing (Wendell R. Garner), somesthesis (G. R. Wendt), and comparative and physiological psychology (Henry W. Nissen and Josephine Semmes) should prove of especial interest to biologists.

The chapter on vision is particularly well written and contains a diversity of unusually interesting recent developments in the field. The material is integrated without apparent loss of detail or accuracy to form the most coherent, impartial, and absorbing summary in the book. The section devoted to hearing betrays a moderate theoretical bias in some parts which does not, however, interfere with the impartiality of the coverage. The somesthesis chapter suffers more from the wide range of material pertinent to this heading and from the paucity of good research in the area than from the author's efforts. The discussion of comparative and physiological psychology suffers a certain amount of narrowness, partly because the subject matter overlaps with that of several other chapters and is contained in these, and partly because the authors express a desire to review only those topics omitted or neglected in previous volumes. Most of the discussion of comparative behavior is devoted to the work of European scholars (Tinbergen, Baerends, et al.) whose work has only recently attracted a deserved attention in this country. The reader may feel, along with me, some disappointment in the open neglect of this fertile and fascinating field on the part of most present-day psychologists, a neglect pointedly reflected in the sparsity of reports in this section. A succeeding section reviews neurophysiology and neural mechanisms in behavior at somewhat greater length, and the chapter concludes with a short but interesting review of neural mechanisms in higher mental processes.

Some readers in biology may also be interested in the competent review of learning by Harry F. Harlow, which justifiably expresses some dissatisfaction with the enormous burden of theory encumbering this area.

The volume on the whole provides at least adequate coverage of the major topics of modern experimental psychology as well as extensive bibliographies for those whose interests will carry them further. It will be particularly useful for a rapid review for those with some previous acquaintance with psychology, and in indicating the trends now developing within the science.

O. THOMAS LAW

EXPERIMENTAL PSYCHOLOGY: *An Introduction.* *By Leo Postman and James P. Egan; edited by Gardner Murphy. Harper & Brothers, New York.* $4.50. xvi + 520 pp.; ill. 1949.

This textbook is a survey of the most important facts in certain selected areas of experimental psychology. It devotes 9 of its 20 chapters to sensation and perception, 6 to learning and memory, and one each to judgment, reaction time and association, emotional behavior, and behavior in social situations. Thirty experiments which can be carried out with minimal materials in an experimental psychology course are described. The writing is clear and straightforward, and intelligible to any student who has had a good introductory psychology course. The book is attractive, and liberally supplied with graphs and other illustrations.

Now that experimental psychology has a new handbook, the need for an experimental psychology textbook which can double as a handbook is much less than it was in 1938, when Woodworth's classic *Experimental Psychology*, still for many purposes the best in its field, was published. Nevertheless, Postman and Egan depart too far from the handbook approach. They give exceedingly few references in the body of the text. Most facts and even most theories are presented dogmatically, with little or no mention of the men responsible for them or the qualifications with which they should be surrounded. The references at the end of each chapter might start a student into the original literature, but would not get him very far. It seems very likely that equally good pedagogy could be combined with a great deal more documentation.

In spite of inadequate documentation, this textbook should serve very well its intended purpose: to be a convenient summary of the basic fields covered in experimental psychology courses, a framework upon which an instructor can hang a more specific and detailed treatment of those issues and experiments which he considers particularly important.

WARD EDWARDS

PSYCHOLOGY AND THE INDUSTRIAL WORKER. *By E. G. Chambers. Cambridge University Press, New York and London.* $2.50. viii + 190 pp. 1951.

This small volume, written in non-technical style, has as its theme the motivating interest of the individual worker in carrying on his trade or profession in relation to his other intrinsic life satisfactions. The author, writing largely from the point of view of the British psychologist, assumes that work is the opposite of a curse to mankind; work rather gives a man the opportunity to channel the physical and mental powers in his nature which have an instinctive root, into purposive effort. "As long as man is capable of the tasks laid upon him and is interested in the nature of the activities involved and the end to be achieved he will be reasonably contented as far as the work itself is concerned."

Man also has outside social and psychological needs to be satisfied, which are however not unrelated to his

work efforts. The province of the psychologist working in the industrial field lies in the area both of individual differences in interests and satisfactions, and also in individual differences in abilities and aptitudes. His job is twofold: (1) to arrange for men to do that work which interests them and is within their capacities; and (2) to take steps in the industrial organization which will take account of the needs of the human organism. Admittedly the psychologist can only recommend to industry, not enforce. What he recommends may be rejected and non-psychological methods continued. However, the efforts of the industrial psychologist impress industrial leaders in that they tend to assist them in such critical problems as high labor turnover, high sickness and accident rates, disloyalty, and strikes. The major part of the book is devoted to analyzing the techniques which the industrial psychologist draws upon to bring about better adjustment of the worker. While not startling in formulating new approaches to industrial problems, the volume is stimulating and well worth reading to bring into focus an over-all perspective of the psychologist's contribution in this field.

MARION R. BARTLETT

MODERN ABNORMAL PSYCHOLOGY. *A Symposium. Edited by W. H. Mikesell. Philosophical Library, New York.* $10.00. xii + 880 pp.; ill. 1950.

This seems to be a rather thorough account of the history and development of the modern psychology of mental illness by a group of competent contributors. The treatment of psychopathology is essentially Freudian in nature but gives an up-to-date account of recent developments in the field and of differences of opinion. For a quick review of any one of the numerous topics, running from the conception of the normal and abnormal to mental hygiene, one will find here fair enough statements of the current situation, even if the book is somewhat weighted in the psychoanalytic direction.

WENDELL MUNCIE

MR. CARLYLE MY PATIENT. *A Psychosomatic Biography.*

By James L. Halliday. Grune & Stratton, New York. $3.50. xiv + 227 pp. 1950.

Halliday has examined Carlyle's published works and some biographical materials and has satisfied himself that he can take the facts recorded and use the "*surmises*", "*guesses*," "*inferences*," "*analysis*," "*interpretation*," and "*suggestions*" based on them as premises. (The words in italics are all taken from one paragraph.) The conclusion reached seems satisfactory enough to its author, but it appears to me that such work, though interesting as speculation and mental exercise, is of little worth as a scientific study.

It seems an unduly naive belief that Carlyle's diagnosis of the social health of Britain during the nineteenth century was more accurate than that of his contemporaries *because* of a "projection of his own psychological sickness [the disintegration of his "inner society"] upon outer society" rather than as a result of observation and intellectual analysis—a sort of triumph of bowels over brain. Throughout the age in which Carlyle lived, the entire social system of the world was changing. Surely one is entitled to believe that that would interest a man of Carlyle's intellect, whether or not he was an anal person.

This interpretation of the recorded thoughts of Carlyle by Halliday inevitably suffers from the simple defect (common to all similar writings) that Halliday's associations are not Carlyle's. Carlyle's autobiographical work was not written without an awareness that it would be scrutinized by others and was not uncensored, that its words were undoubtedly chosen with due respect for euphony and literary form. It follows that we read here certain of Carlyle's thoughts written with a view to eventual public scrutiny, projected through Halliday and inevitably bent to fit conclusions satisfactory to Dr. Halliday himself. Such a method of analysis could equally well be made to support any other psychological interpretation.

The book is well written and pleasant to read. It can be regarded as an interesting essay in a current psychoanalytic style, probably of far more use as an exposition of Dr. Halliday and his psyche than of his "patient, Mr. Carlyle."

DAVID ROSS

THE MASK OF SANITY. *An Attempt to Clarify Some Issues about the So-Called Psychopathic Personality. Second Edition.*

By Hervey Cleckley. C. V. Mosby Co., St. Louis. $6.50. 569 pp. 1950.

The second edition of this well-known book dealing with the psychopathic personality has been rather fully rewritten, and I know of no more stirring presentation of the clinical picture, the social consequences, and the therapeutic difficulties incident to the problem of the psychopath. Cleckley presents clinical cases showing the rich and protean manifestations of the difficulty. He tracks down the pertinent observations, or rather speculations, concerning causation and treatment, and deservedly feels no particular embarrassment at having arrived at no very convincing or particularly helpful formulation of the material. If anybody has done better in exposing our general ignorance and our lack of help-

ful concepts concerning this problem, I do not know who he is. The author's significant contribution, beyond the classical delineation of the various aspects of the problem, lies in offering the concept of a disorder as a semantic one. As Freud described perversion and neurosis as in some way the reverse of each other, the author suggests that psychosis and a psychopathic personality are somehow reverse disorders. Some critical remarks about Alexander's view of the psychopathic personality as a neurotic character are felt by Cleckley to offer some helpful suggestions, but not to cover the problem adequately. On the other hand, he comes to no set conclusions, so far as I can make out, concerning the involvement of the "constitution" here, an issue so widely debated. In passing I note that his summary of my own view (as expressed in my textbook) is not accurate, and one may only hope that in his summary of the views of others, he achieves a greater degree of accuracy.

The book is a very well-organized, well-written text on the subject, and thoroughly to be recommended. Yet I for one get no great help out of viewing the matter as a semantic one. For all the work put into this text, we are still left with the classical problem: Are such people born that way or do they get that way through early experience; and if the latter be true, are there any really effective ways of modifying it; and what are the limits within which modification can be expected—limits, for example, to the age at which therapeutically important life experiences occur to the significance of the habitual behavior patterns of the parents, siblings, and so forth. These practical issues remain, and I am sure the author would agree that solutions to them are not materially advanced by his considerations. But it may also be said that they are lucidly presented in all the stark reality of their personal and social importance.

WENDELL MUNCIE

PSYCHOTHERAPY WITH SCHIZOPHRENICS. *Monograph Series on Schizophrenia, No. 3.*

Edited by Eugene B. Brody and Frederick C. Redlich; introduction by Robert P. Knight. International Universities Press, New York. $4.00. 246 pp. 1952.

This volume, embodying the papers presented at the Conference on Psychotherapy with Schizophrenic Patients, held at Yale University on December 6, 1950, together with the discussions of the papers, and including some additional papers, constitutes a notable symposium on the subject. The principal papers included in the book are by Redlich, Brody, Fromm-Reichmann, Eissler, Lidz and Lidz, Wexler, and Frank. Discussants included Arlow, Wright, Bak, Eidelberg, Semrad, and Kubie.

After concluding this volume, the reader realizes that the psychotherapeutic aspects of the problem of schizophrenia have been given a modernistic refurbishing in the light of some current theories about the process. Redlich's and Brody's presentations form a very telling summary of all the important work of recent years covering the concept and treatment of schizophrenia. Reichmann, Eissler, Lidz and Lidz, and Wexler discuss essentially individual treatment of schizophrenics, and Frank concentrates on the group therapy in hospitalized schizophrenics. No one comes up with any set conclusions about how to achieve the best results.

The most illuminating article is Frieda Fromm-Reichmann's. Out of her rich experience with psychotherapy in schizophrenics, she has much sound advice to give concerning what to expect in the patient-physician interrelationship, how to understand the patient's non-verbal communication, the difficulties in interpretation, and the pitfalls in the concentration on elucidation of content. She makes a strong bid for support for the patient's ego, and for concentration of the attack on the normal reality-oriented aspects of the patient's personality.

Two final impressions come from reading this most stimulating book. (1) A great many serious, psychoanalytically-trained workers are turning their attention to a problem which for too long was thought to be outside the realm of psychoanalytic interest. The whole problem stands to gain from the use of psychoanalytic insights and methods, however modified. (2) The enthusiastic participants very often hail their observations as something utterly new, apparently out of sheer ignorance of what clinical psychiatry has known for long years. David Wright, for instance, finds it necessary to chide Dr. Fromm-Reichmann a bit because of her implication that "classical psychiatry" taught that the verbal and non-verbal communications of schizophrenic patients could not be understood. This just is not so. It seems too bad at this stage of our development to point out that there was a very considerable body of psychiatric knowledge before Freud entered the field. Nevertheless, it is heartening to see serious workers using special concepts, applying their skills to the most difficult field in psychiatry. I have often likened the preoccupation of the psychiatrist with the neuroses, and the corresponding neglect of the schizophrenics, to what would be a wholly ridiculous situation if this sort of thing were translated into the surgical field where practically all of the surgeons would be giving their best efforts and talents to cosmetic surgery, leaving the serious conditions, particularly cancer, untouched. Cosmetic psychiatry, which is what most practicing psychiatry has amounted to for a long time, is very intriguing and enlightening and helpful, but let us not forget that the psychiatric cancer of today, as always, is schizophrenia. The hardy souls who make even the slightest impression on this field must be congratulated for their fortitude, and their willingness to work in a field offering little day-to-day gratification.

It would be unfortunate, however, at this stage, if the problem of schizophrenia were to be reduced by the current enthusiasts into terms of interpersonal relationships as if this were the final and all-embracing basis for psychiatric difficulties.

My critical comments should not serve to leave the impression that I find this volume disappointing. It is one of the most stimulating and rewarding publications on the subject that I know of, and should be in the library of every serious student to be read and reread.

WENDELL MUNCIE

HUMAN BIOLOGY

THIS IS RACE.

Edited by Earl W. Count. Henry Schuman, New York. $7.50. xviii + 747 pp. 1950.

This mammoth anthology contains an excellent collection of what is needed to orient the beginner in anthropology on the subject of what has been thought about race, from Buffon, Kant, and Blumenbach to Hooton, Coon, Shapiro, Boyd, Howells, Wiener, and Washburn. All points of view seem to be represented, and all the great (or pseudo-great) names are here. It is a fascinating parade of opinion, not the least instructive aspect of which is the capacity of workers in this field to become literally possessed by an idea to the exclusion of all balance. It is a most hopeful sign for the future of a field which interests all of us, whether we work in it or not, that most modern anthropologists seem to have gotten away from the hypnotic single idea that possessed men like Retzius (skull measurement), Ellsworth Huntington (force of the environment), or Bolk (fetalization). The 60 contributors are too many to list, but may be divided into 5 of the 18th century, to the fall of Napoleon; 6 of the earlier 19th century, to Darwin; 17 from Darwin to the First World War; and 32 of the 20th century since the First World War. Copious notes and an index make this a reference work truly worth having.

BENTLEY GLASS

EARLY MAN IN AMERICA. *A Study in Prehistory. By E. H. Sellards. University of Texas Press, Austin.* $4.50. xvi + 211 pp., ill. 1952.

This subject is a lively one and extremely interesting, not only in its own right and for its influence on the adjacent fields of geologic chronology, paleontology, paleobotany, climatic change, geomorphology, etc. It is also an example in the history of science. Sellards, now approaching 80 years of age, has played an important part in a crucial period of this subject. He is known for his early discoveries and fearless advocacy of the presence of early man in Florida, and at a time when his efforts brought him little but ridicule. His interest has never flagged, however, and his Florida work is being validated by modern workers.

The introduction gives a brief background on geologic time divisions, the means of age determinations, and the conditions of occurrence and preservation of records of early man. Discussion then turns to the early hunters of the Great Plains. In this area much evidence of elephant and man are found. The finds are often associated with beds of small lakes, with evidences of sequences of climatic change implied by both sedimentary and paleontologic records. The contemporaneity of man and elephant seems little disputed now, though when Sellards began reporting such finds in Florida in 1916, they were thrown out of court.

Sellards next deals with the Bison Hunters of the plains. Early finds of extinct bison were made at the end of the 19th century, but recognition of the validity and significance of such material was delayed more than 30 years, when classic Folsom points were found in such association. This began the slow awakening. Since then a number of such sites have been found.

Discussion turns from the Plains to other regions. Some of the sites of the mountainous West are reviewed, such as Gypsum Cave, Ventana Cave, Sandia Cave, and others. On the Atlantic Coast the Vero-Melbourne, Florida, sites are reviewed. So rapidly is this field of study advancing that new data are available even since this book was published. In general, reconstruction of the human skulls shows the Florida men to be of paleo-Indian type, and chemical tests show the human and extinct animal bones to have comparable degrees of alteration, with the implication of comparable age. Other sites discussed in this region are at Natchez, Mississippi, and at Friesenhahn and Kincaid, Texas. Three sites in South America are discussed. All of these discussions are brief. They will be of greatest interest to the men outside this field and wanting a quick summary.

The origins of Indian cultures are discussed, and the point is made that the fluted points of the great hunters may well be of American origin. The large mammals of the Pleistocene and the problem of their extinction are briefly reviewed. There is a section on general considerations, a guide to the literature, and a very useful 50-page bibliography.

GEORGE F. CARTER

SEXUAL LIFE IN ANCIENT ROME.

By Otto Kiefer. Barnes & Noble, New York. $5.00. x + 379 pp. + 12 pl. 1951.

SEXUAL LIFE IN ANCIENT GREECE.

By Hans Licht. Barnes & Noble, New York. $5.00. xv + 557 pp. + 31 pl. 1952.

Neither of these two books is a recent piece of work.

Both are translations from German volumes, the former having been translated by 1934, the latter by 1930. One is therefore led to wonder whether the publishing of these books at the present date is not simply an effort to capitalize on the general popularity of books dealing with this subject. Such an assumption may, however, be doing the publishers an injustice for, in and of themselves, the volumes are excellent examples of scholarly treatment of a subject which, very easily, could have been handled in a manner designed to capture the eye of the vulgar public. As it is, both volumes have an air of dignified authority, and in no instance has any attempt been made to treat the subject matter except in a very objective way. Essentially, the books deal with sex in its many aspects of family, class, and national expression. Obviously these must be interpretative considerations, but in all instances the source of the material is given. The pages therefore abound with classical references, and the authors have given credit freely where such has been possible. It should be quite clear to anyone who reads these books that sex, from one century to another, varies but little in its expression; only social approbation or condemnation varies.

C. P. SWANSON

YOUR CHILD'S DEVELOPMENT AND GUIDANCE. *Told in Pictures.*

By Lois Hayden Meek. J. B. Lippincott Company, Philadelphia and New York. $3.00. viii + 163 pp. + 1 pl.; text ill. 1951.

THE CHILD IN HEALTH AND DISEASE. *A Textbook for Students and Practitioners of Medicine. Second Edition.*

By Clifford G. Grulee and R. Cannon Eley. The Williams & Wilkins Company, Baltimore. $15.00. xvi + 1255 pp.; ill. 1952.

THE PSYCHOLOGY OF EXCEPTIONAL CHILDREN. *Revised Edition.*

By Karl C. Garrison. The Ronald Press Company, New York. $4.50. xviii + 517 pp.; ill. 1950.

AN INTRODUCTION TO CHILD STUDY. *3rd Edition.*

By Ruth Strang. Macmillan Co., New York. $4.75. xii + 705 pp. + 13 pl. 1951.

THEY LEARN WHAT THEY LIVE. *Prejudice in young children.*

By Helen G. Trager and Marian Radke Yarrow. Harper & Brothers, New York. $4.50. xvii + 392 pp. 1952.

BIOMETRY

AN INTRODUCTION TO STATISTICS.

By Charles E. Clark. John Wiley & Sons, New York; Chapman & Hall, London. $4.25. x + 266 pp.; ill. 1953.

ELEMENTARY STATISTICS WITH APPLICATIONS IN MEDICINE.

By Frederick E. Croxton. Prentice-Hall, New York. $7.50. vii + 376 pp.; ill. 1953.

ELEMENTS OF STATISTICAL METHOD. *Third Edition.*

By Albert E. Waugh. McGraw-Hill Book Company, New York, Toronto, and London. $5.50. xvi + 531 pp.; ill. 1952.

ELEMENTARY PRINCIPLES OF STATISTICS.

By A. C. Rosander. D. Van Nostrand Company, Toronto, New York, and London. $6.00. x + 693 pp.; ill. 1951.

Preference for a statistics textbook is very much a matter of taste, and the non-mathematician must find the approach which makes sense to him. The above books all treat statistics from a "minimum mathematics" point of view. None uses calculus, and all are good introductions to the field. Clark proceeds from a basis in probability which he provides in the first 3 chapters, and is perhaps slightly more sophisticated than the three others. Croxton draws heavily on biological examples, has a good treatment of the presentation of data, and a pleasant style. Waugh is firmly elementary and patient with the reader. Rosander's treatment of analysis of variance is fairly extensive, though all of the books treat that subject. All discuss the *t* test, *F* ratio, chi-square, and correlation. None of these books is a necessity for anyone who already has a book on statistics.

PAUL CRANEFIELD

DE OMNIBUS REBUS ET QUIBUSDAM ALIIS

THE ORIGIN OF THE EARTH.

By W. M. Smart. Cambridge University Press, London and New York. $2.75. viii + 239 pp. + 8 pl. 1951.

Biologists, especially those who are concerned with the study of organic evolution, cannot fail to be interested in the dicta of physicists and astronomers about the age of the universe and our own particular corner of it, or regarding how and when it came into existence and how it has since evolved. All such will turn with welcome to this well-written book by the British astronomer W. M. Smart. The author's style lacks the poetic flights of Jeans and the philosophical aura of Eddington, but it is nonetheless a model of good scientific writing, clear and straight-forward, and it has what the author's eminent predecessors sometimes lacked, good balance. Here one can compare present-day theories and weigh the evidence supporting each one.

Especially interesting are those chapters that deal with the conflict of views between the geologists, who argued for an earth a billion years old, and Kelvin, who thought the evidence pointed to an age of only 100 mil-

lion years or less; the development of radioactive dating; and the more modern impasse between the radioactive evidence of an earth about 3 billions of years old, on the one hand, and a universe that according to the astronomers could scarcely be any older than that itself. The discussion of the succession of theories regarding the origin of the solar system, from the nebular hypothesis to the early 20th century theories of planetesimals or of collision between stars, and finally to the most recent theories of the nebular-cloud (Weizsacker) or electromagnetic forces (Alfven), is also very well presented. Now that, within the past few months, the universe has been doubled in size and the rate of its expansion cut in half, some of this will require rewriting, a task we may fully expect to be competently performed by the author.

BENTLEY GLASS

CENTENNIAL. *Collected Papers Presented at the Centennial Celebration, American Association for the Advancement of Science, Washington, D. C., September 13–17, 1948.*

By 43 Contributors. The American Association for the Advancement of Science, Washington. $5.00. vi + 313 pp.; ill. 1950.

This book commemorates the Centennial Celebration of the American Association for the Advancement of Science and is a collection of papers and lectures which were given at that session by invited speakers. Some 13 general symposia were held dealing with the following general themes: Sciences of Society, Educational Potentials, Human Individuality, Nutrition, Housing, World Health Problems, Sources of Energy, The Upper Atmosphere, The World's Natural Resources, Genes and Cytoplasm, High Polymers, Interactions of Matter and Radiation, Waves and Rhythms.

After reading these papers, I was particularly sorry I had not attended the meetings. I would have liked especially to have heard a number of speakers, such as W. S. Thompson, discuss the subject of Science and Control of Human Population, and his critical evaluation of our failure to use physical and biological sciences to obtain the knowledge that could make for a better and more secure human society; D. M. Bonner's talk on Genes as a Determiner of Cellular Biochemistry, and H. C. Urey on Oxygen Isotopes in Nature and in the Laboratory, to mention but a few. On the other hand, it is quite likely that many of the lectures printed here were considerably improved over their oral presentation by the simple process of having the authors prepare them for publication. In any event, most of the articles have wide appeal and undoubtedly are of sufficient interest to warrant readers in purchasing this book for their personal libraries.

DAVID B. TYLER

THE SCIENTISTS LOOK AT OUR WORLD. *The Benjamin Franklin Lectures of the University of Pennsylvania. Fourth Series.*

By W. V. Houston, W. Albert Noyes, Jr., Curt Stern, Alan Gregg, and Wendell H. Camp; Introduction by John M. Fogg, Jr. (ed.). University of Pennsylvania Press, Philadelphia. $3.00. iv + 147 pp. 1952.

For the 1951 series of Benjamin Franklin lectures, the University of Pennsylvania invited five leading scientists to discuss the long-range significance of advances in their respective fields. Physicist W. V. Houston, chemist W. A. Noyes, Jr., geneticist Curt Stern, physician Alan Gregg, and plantsman W. H. Camp all recognized a growing need for greater public awareness of scientific work and support for it, and the responsibility of the scientist as a member of society. The physicist reflected that only "within the last ten years physics has moved from a position of relative obscurity to one of uneasy prominence," by dealing chiefly with the changing concepts in his field, most of which have not yet reached public attention. The chemist was concerned with the many potential uses for men trained in chemistry, and the need for using them where their contributions would significantly help mankind. The physician felt encouraged by medical progress and the ever broader spread of medical care, but deplored the inadequate support for institutions to train more physicians. The geneticist was enthusiastic over larger yields of food and greater freedom from disease as gifts of his field to human progress but feared that the practical aspects might detract from progress at the level of pure science, on which further future applications must depend. The botanist expressed alarm over the lack of general appreciation of our present North American situation in regard to land for food and materials and man's effect on his environment, and urged more realistic conservation of our resources.

LORUS J. & MARGERY J. MILNE

SOVIET SCIENCE. *A symposium presented on December 27, 1951 at the Philadelphia meeting of the American Association for the Advancement of Science.*

Edited by Ruth C. Christman; arranged by Conway Zirkle and Howard A. Meyerhoff. American Association for the Advancement of Science, Washington. $1.25. vii + 108 pp. 1952.

All happy families are alike, whereas every unhappy family is unhappy after its own particular fashion, as Tolstoi remarked when he pulled up the curtain on *Anna Karenina*, that classical portrayal of certain problems of the "haute bourgeoisie." Thus it is not surprising to find that Soviet science has its own particular difficulties. The Bolsheviki, one recalls, attempted a solution of the problems of the haute bourgeoisie in a somewhat summary manner, in a nation with

a rather low level of literacy and a correspondingly meager proportion of scientifically educated personnel: one result of this being that the Soviet Union is now facing in a particularly acute form the problems of training as many scientists as possible and of developing a sound relationship between scientific activity and the organization that foots the bills.

The (relatively) happy families in the Soviet scientific community appear to include those of mathematics, physics, and chemistry. Thus J. R. Kline, of the University of Pennsylvania, describes Soviet mathematics as "a most active and fruitful activity where fundamental results are being obtained and where there is no evidence of thought control," although Soviet mathematicians are unable to enjoy "personal contacts with colleagues outside the Soviet Union." J. Turkevitch, of Princeton University, in a short survey of the extensive activities in Soviet physics and chemistry, concludes that these fields are represented by "a well-organized body of well-trained scientists carrying out creditable work in many branches of their subjects in the best tradition of the west." Several authors cite examples of notable 19th century Russian contributions to science which did not become generally known, due to the cultural isolation of Russia at that time, because of travel and language difficulties. These authors imply that it is not unreasonable for the young Soviet regime to emphasize these national achievements of the past.

The biological sciences, however, are faring less well. Th. Dobzhansky recounts the sad plight of Soviet genetics with dignity and with perspective, commenting, however, that "the thirst for knowledge in the population of the USSR is immense, genuine, and touching. Moreover, this esteem for science is by no means confined to an educated minority; it is part of the world outlook of the entire community." Horsley Gantt presents a very enlightening and thought-provoking article on Russian physiology and pathology, which seems to me to be an unusually sensible analysis of the Russian point of view in general terms, and to deserve a wide audience.

The difficulties of biology, as compared to the more exact and simpler sciences of mathematics, physics, and chemistry, seem to stem in part from the fact that biology, a relatively backward science, deals with phenomena which the layman is wont to assume he understands almost, at least, as well as the doctor, whereas the more exact and better developed sciences are relatively immune from this kind of difficulty. Furthermore, Zirkle notes that it is in the realm where physics shades into philosophy that Soviet criticism of "capitalist physics" emerges.

Other authors include I. D. London, R. L. Ackoff, J. S. Joffe, and L. Volin, who respectively discuss the situation in Russian psychology, social science, soil science, and the general problem of scientific and intellectual freedom. Altogether this little volume contains much highly interesting information and thoughtful comment. However, considerably more attention is given to the unhappy family than to the happy ones, so that, however significant the moral, as an over-all picture it does not seem free of distortion Some of the authors have not achieved the point of view well described by Gantt:

"In attempting to appraise Russian medical science, I have tried to avoid a feeling of hostility and nationalism similar to what we condemn in the Soviets, to recognize sound criticism even where clothed in aggressive language, and to explain what they are doing in the light of their, not our, traditions and culture. Often what appears strange in them is familiar or tolerable when we see it in ourselves. . . . Seeing their errors so plainly, we should not be guilty of falling into the same morass. As our government becomes itself a director of research, the Russian example may serve the purpose of indicating pitfalls ahead for us. Neither should we underestimate the scientific contributions from Russia. It behooves us now more than ever to weigh their work objectively, to take account of their important achievements, and to be sure that while we complain of their yielding to authority and their adulteration of science with politics, that we learn from their errors, and that during the next difficult years ahead we preserve a scientific objectivity and clarity of thinking that has characterized the world scientists from Galileo and Newton to Pavlov and Einstein. . . . In the words of Pavlov, 'Only science, the exact science about the human himself, and the most sincere approach to it . . . will deliver man from his present gloom and will purge him from his contemporary shame in the sphere of man's relationship to man'."

EVELYN HOWARD

AN OUTLINE OF SCIENTIFIC CRIMINOLOGY.
By Nigel Morland. Philosophical Library, New York. 284 pp. + 12 pl. 1950.

An exceptionally interesting and readable book dealing with many of the methods employed by modern police laboratories throughout the world. It is replete with many references to famous criminal cases in which the newer methods of crime detection have been applied. The subjects covered are fingerprints, identification of individuals, forensic ballistics, medical jurisprudence, forensic chemistry, documentary evidence and forgeries, cryptography and secret writing, microscope and camera techniques, and the applications of scientific criminology today. The book is apparently the result of the author's wide reading in this field. It contains a very excellent bibliography and has

a fair index. It definitely has a place in the library of those concerned with police methods in crime detection.

DAVID B. TYLER

28 SCIENCE FICTION STORIES.

By H. G. Wells. Dover Publications, New York. $3.95. vii + 915 pp. 1952.

The most amazing thing about the science fiction of H. G. Wells is his combination of stunning foresight, long-winded idealism, and wooden characters. In fact, Wells' people can scarcely be called characters at all—they are puppets set in a Utopian landscape, stereotypes with a wonderful flow of words implying some development of brain, but no other internal organs—no heart, no guts. For this reason Wells' longer stories are the least likely to evoke enthusiasm. The present collection contains one full-length novel, *Men Like Gods*, of late Wellsian vintage (1922) and fully characteristic. There is also *Star Begotten* and *A Story of the Days to Come*, of novelette length. It is the short stories that form the unforgettable cream of plots that enthrall and people that don't matter, setting the tradition of science fiction ever since. The basis of selection of the stories is not given—perhaps it doesn't matter, but there is cause for surprise in the omission of one classic of its kind: *The Time Machine*.

BENTLEY GLASS

THE SCIENTIFIC PAPER. *How to Prepare It. How to Write It. A Handbook for Students and Research Workers in All Branches of Science. 2nd Edition.*

By Sam F. Trelease. Williams & Wilkins Co., Baltimore. $2.50. xii + 163 pp. 1951.

The amount of valuable information in this small volume can only be appreciated by one who has used it. The novice writing his first scientific paper—and just a lot of supposedly mature workers—as well as the inexperienced editor and editorial assistant could spend a few hours going through this book with immense profit. Thereafter it will never be far away.

Some of the rules are arbitrary, but better a rule-of-thumb than the utter inconsistency that marks so many manuscripts submitted to the editor. Most American scientific writers now appear to use tenses, punctuate, and abbreviate purely by instinct, regardless of the view so widely supported by psychologists that instincts in the human species are vestigial, if existent at all. Such a manual as this will hardly ward off the plethora of dangling or misattached participles. Nor will it stir the enthusiasm and loyalty of its users like Fowler's *Modern English Usage*—Trelease is humorless and didactic. Yet in most respects he hits the mark. The revised chapters on the preparation of tables and graphs are particularly good. Only in the suggestions for a system of abbreviations of periodicals does this handbook differ completely from the best accepted usage in scientific circles—for in this case, instead of the widely used *World List* or the *Chemical Abstracts* list, Trelease suggests the *Union List of Serials* as a guide. Most biological journals will not approve.

BENTLEY GLASS

SPANISH-ENGLISH CHEMICAL AND MEDICAL DICTIONARY *Comprising Terms Employed in Medicine, Biochemistry, Surgery, Dentistry, Veterinary, Biology, Pharmacy, Allied Sciences and Related Scientific Equipment.*

By Morris Goldberg. McGraw-Hill Book Company, New York, Toronto, and London. $17.50. xiii + 609 pp. 1952.

This is a companion dictionary to the *English-Spanish Chemical and Medical Dictionary* published in 1947. Although it is designed primarily for those whose knowledge of Spanish is at least at the college level, it may be useful to laymen in various commercial fields. It is apparent that great care has been taken in choosing a precise English equivalent to the Spanish, and common usage has been the deciding factor. This dictionary can be recommended for those desiring one that comprises the terms employed in medicine, surgery, dentistry, veterinary, chemistry, pharmacy, and allied sciences.

DAVID B. TYLER

CARNEGIE INSTITUTION OF WASHINGTON *Year Book No. 50. July 1, 1950–June 30, 1951. With Administrative Reports through December 14, 1951.*

Carnegie Institution of Washington, Washington D. C. $1.00 (paper), $1.50 (cloth). xxxv + 260 pp. + 6 pl. 1951.

Biologists regularly turn to the reports of the Department of Plant Biology, Department of Genetics, and Department of Embryology to get a summary and preview of what the Carnegie workers are doing. Much of what is reported here in preliminary form is not published in complete papers for a number of years.

The plant biologists are continuing work upon *Chlorella* growth and photosynthesis and upon the study by genetic, cytological, and physiological means of climatic races, species, and hybrids in such genera as *Achillea* and *Poa*. The embryologists have continued

their famous descriptive work upon human embryos, and are studying intensively such other aspects as (1) the differentiation of the gonads in the opossum and the experimental reversal of that differentiation by hormones, (2) embryonic pathology, (3) the uterine and placental circulation, and (4) the biochemistry and physiology of uterine muscle. The very active Department of Genetics has much to report in the field of bacterial genetics, and in the investigation of mutable loci in maize, growth and inheritance in bacteriophages, mouse leukemia, and the patterns of organization, biochemical as well as morphological, at the cellular level.

BENTLEY GLASS

THE QUARTERLY REVIEW OF BIOLOGY publishes critical reviews of recent researches in all of the special fields of biological science. The contribution should present a synthesis or digest of the researches and a critical evaluation of them. A mere synopsis of the literature without evaluation or synthesis is not desirable.

Theoretical papers are published occasionally, especially when such papers (1) include a critical synthesis of the literature bearing on the theory and (2) are likely to promote further research in a given field.

The article should be written in concise language, yet in sufficiently non-technical form as to be intelligible not only to specialists in other fields but to the general biologist as well. To this end the article should have a general introduction and a summary which enumerates one by one all of the principal facts and conclusions given in the paper. Interpretative diagrams and schemes are very desirable.

Material ordinarily taking the form of footnotes is set in small print and placed in the text and consequently should be written in a style so as to fit readily into the text. Acknowledgments are printed in the text in small type at the end of the article just preceding the List of Literature. Recent issues of the Quarterly should be examined for style as regards (1) section or subsection headings in the text, (2) literature citations in the text, and (3) List of Literature.

The subjects and authors of articles are selected by the Editors and members of the Advisory Board. Unsolicited articles which conform with the objectives of the Quarterly will be considered for publication.

A feature of the REVIEW is the section dealing with *New Biological Books*. In this department the book literature of different countries in the field of Biology is given prompt notice.

The QUARTERLY REVIEW OF BIOLOGY is issued in March, June, September and December.

Twenty-five reprints, with covers, of articles will be furnished to contributors free of cost. The reprint order blank accompanying galley proofs gives the cost of additional reprints.

Manuscripts may be sent to Dr. B. H. Willier, Department of Biology, The Johns Hopkins University, Baltimore 18, Maryland.

Books for Review may be sent to Dr. H. B. Glass, Department of Biology, The Johns Hopkins University, Baltimore 18, Maryland.

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